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PROGRESS IN BIOLOGICAL INQUIRIES
1926

INCLUDING PROCEEDINGS OF THE DIVISIONAL CONFERENCE JANUARY 4 TO 7, 1927

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By ELMER HIGGINS

DEPARTMENT OF COMMERCE
Bureau of Fisheries Document No. 1029

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DEPARTMENT OF COMMERCE
BUREAU OF FISHERIES
HENRY O'MALLEY, Commissioner

PROGRESS IN BIOLOGICAL INQUIRIES 1926

INCLUDING PROCEEDINGS OF THE DIVISIONAL CONFERENCE JANUARY 4 TO 7, 1927

By ELMER HIGGINS
Assistant in Charge of Scientific Inquiry

APPENDIX VII TO THE REPORT OF THE U. S. COMMISSIONER OF FISHERIES FOR 1927

Bureau of Fisheries Document No. 1029

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PROGRESS IN BIOLOGICAL INQUIRIES, 1926[1]

INCLUDING THE PROCEEDINGS OF THE DIVISIONAL CONFERENCE, JANUARY 4 TO 7, 1927

By Elmer Higgins

Assistant in Charge of Scientific Inquiry

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FOREWORD

As indicated by the table of contents, the following report of the division of scientific inquiry is presented in two parts: Part I is a condensed statement of the chief results of the investigations conducted by the division during the calendar year; Part II is a transcript of the proceedings of the divisional conference held January 4 to 7, 1927. While both parts treat of the work of the division, it will be seen that the accounts of the various investigations, as given in the proceedings of the conference, are less detailed in nature but include the general and historical bearings of the problems, in most cases stressing the practical significance of results. The accounts of the investigations given in the first section are more nearly restricted to the detailed results obtained during the current year, and therefore supplement the more general treatment.

The divisional conference of January, 1927, was the first meeting of its kind ever held by the Bureau of Fisheries. There have been many fisheries conventions, in which the bureau's investigators have participated; there have been frequent conferences between the officers of the bureau and smaller groups of workers interested in particular problems; but never before, except, perhaps in the early days of the Fish Commission, has the entire staff of the division of inquiry been assembled for a general consideration of its problems. The undertaking was an experiment, therefore, and it yet remains to be determined if the experiment be a success or if it will be repeated. It can hardly be doubted, however, that the efficiency of the bureau's work will be increased materially by overcoming the effects of the isolation that surrounds the field investigators by bringing them into contact with others in the same and associated fields, by the mutual exchange of ideas, and by the friendly criticism of their fellows on the staff. General satisfaction and approval were expressed by all who were in attendance.

The conference was planned for the midwinter season, when field work is at a minimum; and the investigators' regular visits to Washington, which formerly were scattered through the year, were delayed or hastened in order to bring the staff together at one time. The meeting of the advisory committee on scientific investigations of the bureau, appointed by Secretary Hoover, was also scheduled to occur at this time; and the bureau was further fortunate in having as guests noted visitors of other departments, who were visiting Washington in connection with their own work. Approximately 40 persons took part in the discussions, including the commissioner, deputy commissioner, chiefs of the divisions, investigators of the division of scientific inquiry, members of the advisory committee on scientific investigation, and several invited guests. The program continued through 3½ days, and included many carefully prepared papers, followed by extended and stimulating discussion.

Several evening gatherings of a social nature were arranged privately, also affording opportunity for personal contact and acquaintanceship among members of the staff and materially contributing to the development of a real *esprit de corps.*

Because of the comprehensive treatment of the various fields of investigation by the investigators at the conference, it is considered worth while to publish the transcript of the proceedings. Although most of the discussion was of such an informal nature that it has been deleted editorially, the papers and parts of the discussion are included herewith in the belief that they present a more complete and readable statement of the widespread and complex activities of the division of scientific inquiry in the interests of fishery conservation than could well be presented in the regular report.

Part I.—INVESTIGATIONS CONDUCTED DURING 1926

INTRODUCTION

During 1926 the work of the division of scientific inquiry has shown, in increasing degree, the effect of the conscious and deliberate effort to center attention upon the problems of the fisheries as a distinct branch of marine biology. While none of the lines of investigation under way in 1925 have been abandoned, no effort has been spared to so conduct the work that results of practical benefit to the fisheries and to the fishery administrator may speedily be attained; and it is believed that gratifying progress, comparing well with that of the past and promising much for the future, has been made.

It should not be understood by this that only temporary and insignificant problems, bearing solely on immediate needs, have been undertaken; for it is realized that the big problem that the fishery biologist faces—the problem that enlists his deepest concern and demands his best effort—is the task of conserving or rebuilding a dwindling fish supply. The problem has not always been attacked directly, but in all cases a necessary groundwork has been laid as the basis for more immediately productive studies in the future.

The practical utility of the bureau's investigations is becoming more apparent to the public at large, and with public confidence has come a demand for additional investigations. The Congress and many State fishery departments have responded with greater financial support, and as a result it is confidently believed that the science of fishery husbandry and the conservation of aquatic resources will be advanced more rapidly during the coming years than ever before.

Perhaps the outstanding accomplishments during the past year are the development and application of principles of oyster culture to the varying conditions found on the Atlantic seaboard, the scientific regulation of the Alaska salmon fishery, and the development of aquiculture as applied to fresh-water fishes. Extensive surveys have been made of the oyster industry from Cape Cod through Long Island Sound, and on the southern shore of Long Island, in South Carolina, Georgia, Mississippi, and Texas; and recommendations on which the various States may base an oyster-cultural program have been offered. Hydrobiological conditions differ so greatly in the various localities that different procedures must be followed to increase oyster production. There have been discovered great areas, now barren, which, without doubt, can be utilized for the production of oysters. Other areas, which have been depleted by overfishing, can be restocked; and in many cases the yield and quality of beds now productive can be increased materially.

Not only have these field observations been of aid in drafting general oyster-cultural programs, but fundamental researches upon

the feeding and reproduction of oysters have contributed to our knowledge and made possible the more complete utilization of the present supply and a material increase in reproduction in unfavorable areas or seasons. Thus, through these studies of physiology, the fact of hibernation at a temperature below 41° F. has been reaffirmed;[2] at which time, as feeding is discontinued, it may be possible to market, without danger to the public health, oysters from areas that, during warm weather, may be condemned because of slight pollution. Likewise, through studies of the physiology of reproduction, it has been possible to induce spawning under experimental conditions. The possibility, therefore, is by no means remote that spawning on the natural beds likewise may be induced artificially during seasons when natural spawning would not occur otherwise, thus materially increasing the supply of oysters by insuring an abundant supply of seed. In addition, studies on the ecology and behavior of oyster larvæ have resulted in the designing of highly efficient spat collectors, making it possible, at small cost, to harvest a profitable crop of seed oysters on bottoms where setting does not occur naturally.

Congress has vested the control of the Alaskan fisheries in the Department of Commerce, and the regulations promulgated by the Secretary are administered by the Commissioner of Fisheries. Perhaps in no other locality has it been realized so keenly that fishery regulations must rest upon a solid foundation of biological knowledge; and it has been one of the most important functions of the division of scientific inquiry, with the extensive cooperation of the Alaska division, to conduct the biological researches which make possible the scientific husbanding of the great salmon fisheries. The past year has marked the culmination of a long period of investigation of the biology of the Pacific salmon. As a result of these studies, an understanding of many of the factors that cause the fluctuation in supply has been gained. To be able to predict the degree of abundance of any species of fish in future years is one goal of all fishery investigators, and it now appears that eventually reliable predictions concerning the magnitude of the runs of salmon in certain localities may be made a year or more in advance. The realization of these hopes will not only make possible the fullest control of the supply through regulation of the fishery, but will afford economic protection to the industry in years when poor runs are anticipated, which should result in the saving of millions of dollars.

Gratifying results in the newly initiated work in aquiculture also have been obtained. The first year's pond experiments at Fairport, in which the ponds were fertilized to increase plankton production and in which various plankton-feeding species of fish were used as forage for the game species, has resulted in a marked increase in total production. Results of equal promise have followed the bureau's investigations in the treatment and control of diseases of hatchery-reared fish. Because of the greater demand in recent years for larger fingerlings for stocking streams and lakes, fish-culturists have attempted to rear their stocks to larger sizes. The effectiveness of the hundreds of hatcheries throughout the country has been les-

[2] Based on experiments initiated before, but completed after, the end of the calendar year 1926.

sened materially, however, by the heavy mortality of the older fish. The bureau's efforts, therefore, are of great importance, for its studies on nutrition and diseases have made possible an increased output of the larger fingerlings from hatcheries that formerly suffered the ravages of disease.

Studies of the Atlantic marine fisheries have been prosecuted with increased vigor. Contributions to the basic knowledge of the biology of the region have been made through studies of the fish fauna, plankton, and oceanography of the Gulf of Maine; the fauna of Chesapeake Bay; cod-egg production in Massachusetts Bay; and migrations of the adult cod on the shore and offshore banks. The key to the fluctuation in the mackerel supply also has been discovered in the phenomenon of dominant year classes. These investigations disclose the fact that the mackerel in the tremendous catches of the past two years are the progeny of but two successful spawning years. It is believed that continued observations will make possible the forecasting of the immediate future success or failure of the mackerel fishery.

The extent and effectiveness of the bureau's investigations has been increased materially by the cooperation of various State departments. During the past year, work was conducted in cooperation with the States of North Carolina, Texas, Arkansas, Minnesota, Michigan, Wisconsin, Washington, Oregon, and California. The States provided men, laboratories, boats, and other equipment, and have been enthusiastic over the results obtained.

An important conference of the North American Committee on Fishery Investigations was held at St. Johns, Newfoundland, on July 9, 1926, for the purpose of further coordinating the efforts of the several nations interested in the fisheries of the North Atlantic region. Dr. H. B. Bigelow, Elmer Higgins, and O. E. Sette represented the United States; W. A. Found, Dr. A. H. Leim, and H. E. Tanner represented Canada; and D. James Davies, A. C. Goodrich, and G. F. Sleggs were the Newfoundland representatives. No representatives from France or Portugal were present. The various fishery investigations were considered by the committee and recommendations of extension or modification were made in some cases. The importance of statistics in the codfishery of the entire region was dwelt upon, and plans for participation by Newfoundland in the investigation of the oceanography of that region were made.

Undoubtedly the most serious handicap to the proper development of effective fishery conservation in the United States is the almost uniform lack of adequate statistics of the fisheries. While there has always been a more or less general realization of the economic value of records of total annual yield, the numbers of persons engaged, and the amount of investment in the fisheries, less attention has been given to the need for determining the relative abundance of the fish stock, year by year, as an indication of the state of the fisheries. It has been announced repeatedly that the aim of the division of scientific inquiry is to study fluctuations in the fisheries and to determine their immediate causes; but in nearly every case it has been impossible to attack the problem from a quantitative standpoint, which is by far the most important aspect, because of the lack of suitable statistical data. The yield per unit of effort, such as the boat

catch per day, has long been recognized as a useful index of abundance of fish in the sea, but the present records can not be analyzed on that basis. Suitable statistics must contain these essential details; they must be uniform throughout the range of a fishery, continuous, free from bias, and stored in central places in such a way that they may be preserved for subsequent analysis by fishery investigators. Only the State governments have authority to enforce the collection of these records; and inasmuch as the various States divide jurisdiction over the shore and lake fisheries, it is necessary that concerted action be taken and that some central agency supervise and coordinate the States' activities in this direction. It seems essential, therefore, in order to obtain that intimate knowledge of fish life upon which all rational regulation must depend, that the Bureau of Fisheries should take a leading part in bringing to the attention of the State governments this need, and in advising them concerning the character of legislation necessary to set up an adequate statistical system. The bureau should serve as the coordinating agency in bringing the statistics together and making them available to the biologist. This the bureau is prepared to do, and it is anticipated that material progress toward the accomplishment of such a program will be made during the coming year.

The following progress reports, covering the most important investigations conducted by the division during the calendar year 1926, were prepared in the main by the investigators in charge.

FISHERIES OF THE ATLANTIC AND GULF COASTS

LIFE HISTORIES AND MIGRATIONS OF COD, POLLOCK, AND HADDOCK

Investigations of the great codfisheries of the New England coast, begun several years ago, were continued. The first method of attack has been to conduct tagging experiments to determine the movements and segregation of the various stocks of cod, pollock, and haddock that inhabit the region, and to study the development and early life history of the eggs and larvæ of these fishes.

Tagging operations in 1926 received a late start, owing to the conditioning of the newly acquired vessel, *Albatross II*. The *Halcyon*, which has carried on this investigation ever since it was begun in April, 1923, has been taken out of service.

The first cruise of the *Albatross II* was made to the northeast peak of Georges Bank, where, fishing from August 13 to 19, 1,014 cod, 23 pollock, and 66 haddock were tagged. The depth of water was nearly uniform (at 47 fathoms), and it was possible to utilize for tagging about 75 per cent of the total number of fish caught.

The second cruise was directed to southwest Georges Bank and Nantucket Shoals from September 5 to 11, where 1,606 cod, 10 pollock, and 114 haddock were tagged; but of these, only 5 fish were caught on Georges Bank.

The *Albatross II* fished off Mount Desert, Me., from August 20 to 23 and from September 26 to October 1, tagging 945 cod, 6 pollock, and 461 haddock. At the Bureau of Fisheries' Woods Hole biological station 946 cod were tagged on January 6 and 7, 1926. These fish had been held as a brood stock since November, 1925, and after spawning were tagged and liberated from the station's dock.

Exclusive of the Woods Hole fish, 40,097 cod, pollock, and haddock have been caught and tagged at sea from 1923 to 1926. A complete statistical summary follows:

Item	1923	1924	1925	1926
Number of cruises	7	9	16	4
Days of actual fishing	43	51	76	23
Hours of actual fishing	333	318.5	461	167
Number of cod tagged	7,618	6,209	10,420	3,565
Number of pollock tagged	2,215	916	949	39
Number of haddock tagged	411	3,223	3,891	641
Total number of fish tagged	10,244	10,348	15,260	4,245
Average number of fish tagged per day	238	203	201	185
Average number of fish tagged per hour	30.76	32.5	33.1	25.4

According to localities, the number of fish tagged is divided as follows:

Item	1923	1924	1925	1926
Massachusetts, south of Cape Cod	10,231	4,384	6,143	1,730
Massachusetts, north of Cape Cod	13	163	314	0
New Hampshire	0	8	5	0
Maine	0	5,793	8,798	1,412
Georges Bank	0	0	0	1,103

The total number of recaptures up to December 31, 1926, amounted to 1,940 and consisted of 1,742 cod, 72 pollock, and 126 haddock.

Scale samples were taken from all fish tagged in 1926, as well as during 1924 and 1925. All the samples taken during 1924 (over 10,000) have been mounted and about 1,000 have been studied.

Although tagging operations during 1926 were not as extensive as during the preceding three years, some very instructive results were obtained. It is believed that the year 1927 will produce results of great interest to this investigation, for not only is it planned to operate on an enlarged scale, but many of the experiments begun during the preceding four years will be one year nearer completion and in some cases may be terminated successfully. The outstanding results of the investigation during 1926 are as follows:

1. On the one cruise made to Nantucket Shoals (September 5–11), it was found that the stock of cod differed considerably in size from the fish present there during the period from the beginning of the investigation in 1923 up to 1925. Heretofore, cod less than 18 inches in length were caught rarely, and 22 to 32 inch fish predominated; but in 1926 large fish were scarce, fish below 18 inches were common, and the predominating sizes were 16 to 22 inches. Some intimation of this exodus of large fish and entry of small fish was had in the preceding fall and could be traced to August, 1925, when the bottom-water temperature was 15° C. (59° F.) and the cod were much scattered and bunched. How unusual this high water temperature is can not be known until the investigation has continued further.

In each of the tagging years (1923, 1924, and 1925), a definite migration of tagged cod occurred from Nantucket Shoals to the waters between Rhode Island and southern New Jersey. As the

stock of fish present on Nantucket Shoals during 1926 was composed of much smaller cod than during the preceding years, it was of interest to note how this difference would affect the catches of cod made between Rhode Island and New Jersey during the winter and thereby gain some idea as to how much Nantucket Shoals contributes to the southern migration.

It was found that small cod were far more abundant than usual around Marthas Vineyard during the fall of 1926, enough so to receive special comment in the newspapers and trade magazines. Furthermore, fishing-boat captains along the western Long Island and New Jersey coasts asserted that large cod were not plentiful during the fall of 1926, but that small cod, below 22 inches in length, were much more numerous than for many years past. These data indicate that the Nantucket Shoals stock of cod contributes a large part of the fish that migrate south each fall to the Middle Atlantic States.

2. No recaptures have been reported of the 1,000 cod tagged on northeast Georges Bank during August, 1926. It is probable that these fish have not yet made extensive migrations (it is not certain that they ever will) and that no fishing vessels have been operating within the immediate vicinity of the tagging grounds.

3. As a result of the cod tagging along the shores of eastern Maine during 1924 and 1925, it was found that if these cod migrate at all they go to the Bay of Fundy and occasionally to the eastern coast of Nova Scotia. These same results obtained during 1926.

4. Very little is known definitely concerning the whereabouts and habits of cod and haddock between 1½ and 7 inches in length in the western Atlantic. This year, by means of a 30-foot otter trawl, quite a few cod and haddock, 2 to 5 inches in length, were taken on Georges Bank. The stomach of one Georges Bank pollock contained 12 haddock, 2½ to 4 inches in length, but other pollock examined contained none. Although these data are not extensive, the indications are that young cod and haddock of these sizes (they are less than two years old) are present in large numbers on Georges Bank, Browns Bank, and other offshore fishing grounds.

5. A study of scales has shown that cod off southern Massachusetts grow at the rate of about 7 inches each year for the first three years.

A more complete discussion of the cod problem is given on page 601 of this report.

The investigations of the spawning grounds and the early development and distribution of cod, haddock, and pollock in New England waters, carried on since 1924 by Dr. Charles J. Fish, assisted by Marie P. Fish and Robert A. Goffin, were interrupted during the past year because of the condemnation of the *Fish Hawk* and the necessity for making extensive repairs on the *Albatross II*. Therefore, the material obtained to date has been arranged and examined. It includes two series—one from Massachusetts Bay and one from the region of the Grand Banks.

The immediate problem in Massachusetts Bay involved a determination of the value of that area as a production center and also as a nursery for the large numbers of eggs and larvæ, which we have reason to believe are being carried in constantly from the east. The results to date indicate that local production in Massachusetts

Bay does not maintain the supply of fish in that area, and that in all probability the inshore spawning grounds north of Cape Cod, as a whole, are not selfsupporting but must rely on immigration from other areas. It is to be expected that some of the cod eggs that drift west into the Gulf of Maine will hatch before they leave the bay; but this probably does not alter the situation in any way, for the current that carries them in will transport them out again. A report on this work will be submitted in the near future.

Biological collections from the region of the Grand Banks supplement, to a surprising degree, the hydrographical observations made by the Ice Patrol on the currents in that area. Each of the three principal water masses of the region—the Labrador current, Gulf Stream, and "banks water"—is characterized by its own particular pelagic faunas and can often be defined as accurately by net collections as by temperatures and salinities.

In 1921, a series of hauls taken over the zone where the Labrador current and the Gulf Stream meet indicated clearly the sharp line of demarcation between the Arctic and the Atlantic faunas. The present investigation is limited to the area east of Newfoundland and is concerned with the meeting of the Labrador current and the third great mass—the "banks water."

The reports of the Ice Patrol have shown that a large drift of coastal water moves east from the vicinity of Cape Race and spreads over the whole region of the Grand Banks except the northeastern part. The plankton collections indicate clearly the faunal difference in these two water masses. The most northerly station yielded a true Arctic community, but the more southerly ones, along the western margin of the banks, were of a boreal coastal nature, although far from land.

It may be that the Grand Banks are not dependent entirely upon local production to maintain their supply of bottom invertebrates (fish food) but are constantly enriched by an influx of the pelagic young of species from the coast, which, sinking to the bottom, find a favorable environment in the comparatively shallow water of the banks. If this be true, and both the hydrographical and biological evidence support it, the Grand Banks are unique among fishing grounds in that an offshore drift continually adds to the resident bottom invertebrate community upon which the cod feed, while two great ocean currents transport enormous quantities of microscopic plants and animals, which, killed in the mixing zone of the sudden temperature changes, sink to the bottom and insure a rich source of nourishment for the crabs, upon which the fish feed.

In the course of the investigation several interesting minor observations were made. A great difference was found in the size of cod eggs from Massachusetts Bay and those from the region of the Grand Banks, the former averaging 1.5 millimeters and the latter 1.28 millimeters. The cod eggs collected in the region of the Grand Banks on June 5 to 17, 1924, ranged from 1.1 to 1.55 millimeters, and averaged 1.28 millimeters.

These eggs were found to average between 1.2 and 1.3 millimeters everywhere except at the most northerly station (approximately 390 miles north of the tip of the Grand Banks), where a surprising average of 1.42 millimeters was found, no eggs being

less than 1.35 millimeters. The total absence of cleavage stages at this station (the earliest eggs having the embryo well developed) indicates that they may have been carried for a long distance or belong to an entirely different race of fish. The force of the Labrador current at this point, the low temperature, and the number of planktonic forms that occur here and at no other station tend to substantiate the former possibility.

Although the significance of the differences of the means has not been determined statistically, it appears, on the basis of the collections alone, that temperature has a decided effect on the size of cod eggs, causing them to average larger during the colder periods and decrease in size as the water becomes warmer. Thus in Massachusetts Bay eggs averaged smallest in December and May and largest in February. In the region of the Labrador current the largest average was found at the northernmost station and the smallest at the more southerly ones when the summer warming of the water had become noticeable. In the North Sea a similar condition was noted by Ehrenbaum, who found cod eggs averaging 1.46 millimeters in January and 1.3 millimeters in April.

Following are the average sizes of 50 cod eggs taken in Massachusetts Bay at various times:

Date	Millimeters
Dec. 11, 1924	1.458
Dec. 17, 1924	1.495
Jan. 7, 1925	1.494
Feb. 6, 1925	1.529
Mar. 10, 1925	1.501
Apr. 8, 1925	1.513
May 20, 1925	1.488

The average size of cod eggs from the Grand Banks in June, 1924, was 1.2 to 1.3 millimeters; from the polar current in June, 1924, 1.42 millimeters; and from Massachusetts Bay on June 5, 1926, 1.425 millimeters.

Experiments were carried on at Gloucester and on the Boars Head fishing grounds to determine whether the seasonal fluctuations in the average size of cod eggs are due to temperature. In general the results appear to be significant, the eggs fertilized at 0° C. averaging 1.447 millimeters and those fertilized at about 8° C. averaging 1.4106 millimeters. The increase in size at a reduced temperature corresponds with the observations made on eggs taken in the field.

The investigation in Massachusetts Bay was not confined to the young of the cod, haddock, and pollock, but included eggs and larvæ of all other species taken with them.

There are at least 61 species of fishes that either have been taken in their larval forms about Woods Hole or may be expected in collections there. Thus far a bibliography for each species has been gathered, including original illustrations and notes on distribution, and the missing links in developmental histories have been listed. The collections are being searched at present to fill these gaps.

MACKEREL

During 1926, the investigations on the mackerel fishery, in cooperation with the division of fishery industries, were continued by

O. E. Sette, assisted by R. A. Nesbit and R. A. Goffin. The program contemplates the simultaneous collection of biological and statistical data and their analysis to provide an understanding of the life history of the species and the fluctuations of the fishery. During the 1926 season an observer was stationed at the principal mackerel receiving ports for the purpose of interviewing mackerel fishermen as fish were landed and securing data on the locality, dates, size of catch, and fishing effort involved, as well as taking measurements, scales, etc. of the mackerel landed. During the season over 1,200 vessel skippers were interviewed, over 25,000 mackerel were measured, and about 3,500 scale samples were taken. Other data were collected at Woods Hole, Mass.

Analyses of the 1925 and 1926 data on sizes and ages have proceeded as rapidly as possible with the present limited personnel. Although they are yet far from complete it is now fairly certain that the unusually large catches of 1925 and 1926 were composed largely of mackerel that originated in one spawning season, provisionally determined to be that of 1923. The preliminary age analysis of the stock of mackerel present during the past two years leads us to believe, further, that the 1921 spawning season was more than normally successful; that the 1922 season was practically a failure; that the 1923 season was extraordinarily successful; and the 1924 season was probably average in production. This at once gives the clue to one cause of fluctuations in the mackerely fishery—that is, unequal increments in various years. It is believed that continued observations of this sort eventually will permit the forecasting of immediate future success or failure of the mackerel fishery.

The steamer *Gannet* was detailed to mackerel investigations for about five weeks during the spawning season in Massachusetts Bay, and was engaged in making tow-net hauls and taking oceanographic data. An incomplete analysis of the plankton collected showed that an abundance of mackerel eggs and larvæ was present during and immediately after the spawning season. This is significant, inasmuch as very few mackerel eggs and no larvæ had been found previously in the Gulf of Maine. It had been thought heretofore that the Gulf of St. Lawrence, where large quantities of mackerel eggs and larvæ had been taken by the *Princess* and *Acadia* in the Canadian fisheries expedition of 1914–15, and again on the *Cheticamp* expedition of 1917, was the only important spawning ground for American mackerel. The 1926 cruise in Massachusetts Bay demonstrated the presence of comparable quantities of eggs and larvæ in this locality, thus extending our knowledge of the mackerel spawning grounds. It is possible that more extensive work of this nature may indicate even greater ranges for important breeding places of the mackerel.

Tagging operations in 1926 were very limited, only 599 fish being tagged off the coast of Delaware in April. Two of these fish were recaptured, one of them on the date of tagging at virtually the place of release. The other was retaken in August off Cape Cod. Twelve fish tagged in 1925 were reported recaptured during 1926. It will be recalled that all of the mackerel tagged in 1925 were released along the coast of New England, and the recaptures during the same season indicated a general spread of mackerel from the points of release. The recaptures in the next year (1926) occurred from Fire

Island to Gloucester, most of them to the southwestward of the place of release. The tagging operations have been too limited to warrant any definite conclusions. It is hoped to continue them in 1927, when a more suitable tag may be devised.

SMELTS OF NEW ENGLAND

A general account of the natural history, fish-cultural propagation, and conservation of the Atlantic smelt, with a history of the smelt fisheries, by Dr. William C. Kendall, to which reference was made in a previous report of this division, has been published. That account is being supplemented by another paper on the smelts of the genus Osmerus, which embraces life histories, age, rate of growth, racial peculiarities, etc. The original plan to include Pacific species of the genus has been simplified by a recent reviewer of Pacific smelts, who has relegated all but one species (formerly regarded as Osmerus) to other genera. Work on the smelts is discussed at greater length on page 614.

MULLET

The gray-mullet (*Mugil cephalus*) investigation in the south Atlantic States was continued during 1926 at Beaufort, N. C., by Elmer Higgins and Robert O. Smith. The tagging experiment was extended, 1,000 fish being tagged and liberated at Beaufort during July, August, and September. Because of the difficulty of securing uninjured fish in quantity from commercial hauls, a beach seine was operated by the investigators and crew. This made possible the tagging of mullet and at the same time the tracing of the growth of mullet spawned in the preceding fall. Due to various factors, among which might be mentioned more effective advertising, smaller commercial catch, and the different sizes of fish tagged, 34 tags were returned during the year, an average of 3.39 per cent, or one returned for every 29.5 fish tagged. In some cases, 2 to 5 individuals from the same school were returned. All tags were received from North Carolina, except two from the upper portion of the South Carolina coast, thus confirming the results of previous experiments. There were no returns from fish tagged in 1925.

The assumption that so-called "Cape mullet" constitute a distinct division of the local stock seems to be corroborated by analyses of commercial catches during 1926. These fish appear at Beaufort about the middle of September, and are plentiful until the first of November. Length-frequency studies of Cape mullet that were taken in a number of commercial hauls indicate that these fish are intermediate in size between the local O and I classes. However, during the latter part of this season (mid October to mid November) it was virtually impossible to separate local O-class and "Cape" mullet on the basis of average length, inasmuch as some of the commercial Cape mullet averaged extremely small in size.

Study of the growth of mullet, particularly the O-class, was continued and significant results were obtained. Juvenile mullet of 22 millimeters body length appear at Beaufort by the middle of January. They become more abundant during February, March, and April, but there is little growth until the middle of April, when the water temperature reaches about 20° C. In May the rate of growth

increases rapidly and continues, at an average of approximately 2 centimeters per month, through October.

In the hope of observing spawning, a number of large mullet were placed in a tank of running sea water at the fisheries laboratory at Beaufort on October 27. From time to time the roe in killed specimens was examined, and while it appeared to be healthy, the eggs did not mature. The roe had not ripened when the last specimen died on January 14, 1927. Various methods of estimating the number of eggs in the ovaries of a fish of 367 millimeters body length were tried. Different methods gave results varying between 759,000 and 1,526,000. The eggs are not spherical but are more nearly an ovate spheroid, the long axis being 0.61 millimeter and the short axis 0.57 millimeter. In all parts of the ovary the eggs are in the same stage of development. This meager evidence, combined with the observed disappearance of roe mullet from inside waters during October and the compact size group of young fish, leads to the conviction that the spawning season is short.

Observations throughout the season on the selectivity of commercial gear, in conjunction with samples of the commercial catch, show that the strain of the fishery falls most heavily on three age classes: "Cape mullet" approximately 1 year old, 2-year gray mullet that will spawn the following fall, and roe mullet chiefly 3 years old. The yearling stock that appears at Beaufort in the middle of January as individuals less than 1 inch long and attains a total length of about 7 inches by October seldom is taken.

SHORE FISHERIES OF NORTH CAROLINA

Life-history studies of various food fishes have been undertaken at the fisheries biological laboratory at Beaufort, N. C. The eggs of the pigfish (*Orthopristis chrysopterus*) and those of the anchovy (*Anchoviella epsetus*) were secured and their development was studied. The development of the larvæ and the rate of growth of the young fish, too, have been followed. Special attention was given to the food consumed by the young fish and the changes that take place in the diet with age, together with the change in environment chosen by the fish as they develop. Similar studies, relative to the rate of growth, food, and environment of young sheepshead (*Archosargus probatocephalus*), young spot (*Leiostomus xanthurus*), and young white perch (*Bairdiella chrysura*), were undertaken. This work was carried on by Dr. Samuel F. Hildebrand and Irving L. Towers.

Other work at the Beaufort laboratory is discussed elsewhere in this report.

TEXAS MARINE FISHERIES

In accordance with plans outlined in 1925, an investigation of the biology of the important food fishes of coastal Texas has been undertaken. A field station has been established at Corpus Christi, Tex., where John C. Pearson, with the aid of a boat and crew furnished by the State fishery authorities, is making extensive and systematic collections. Variations in the commercial fishery have been fol-

lowed, but so erratic and limited is the fishery in most localities, due to legislative restriction, that few data of value can be secured.

The major facts in the life histories of the three leading food fish—redfish (*Sciænops ocellatus*), spotted trout (*Cynoscion nebulosus*), and drum (*Pogonias cromis*)—are being ascertained. Collections of fish of all sizes, from larval stages to mature adults, have been made regularly over a wide range of localities and afford an understanding of the habits and movements, spawning age, and rates of growth of these valuable species. Preliminary findings of this study are given at greater length in another section of this report (p. 627).

FOOD HABITS OF SHARKS

Further observations on the food and reproductive habits of various sharks that enter into the catch of a commercial shark fishery at Big Pine, Fla., were made during the winter months of the past year. The stomachs and reproductive organs of about 1,500 sharks were examined carefully, with the object of ascertaining, if possible, the actual damage done to food fishes by these scavengers of the sea. Ten species of sharks were observed in the commercial catch, although the larger part of this catch comprised only five species.

The food habits of the various species differed somewhat, but the amount of food fish eaten by all species was found to be very small. Refuse, crustaceans, and nonfood fish seemed to make up the bulk of the food. However, the fact that most sharks do much damage to commercial fishing gear probably warrants their unlimited utilization.

FISHERIES OF THE PACIFIC COAST

The bureau's salmon investigations on the Pacific coast are conducted as an integral part of the program of the International Pacific Salmon Investigation Federation, and hence are closely coordinated with work undertaken by Canada and the Pacific Coast States. The activities of this federation, therefore, are of direct interest to the readers of this report.

The third meeting of the executive committee of the International Pacific Salmon Investigation Federation was held in Seattle on December 2, 1926. At this meeting a program committee, consisting of Dr. W. H. Rich, chairman, Dr. C. H. Gilbert, Dr. W. A. Clemens, of the Biological Board of Canada, and Prof. J. O. Snyder, representing the California Fish and Game Commission, presented a research program that was adopted unanimously by the executive committee. This program is presented in full elsewhere in this report (p. 635).

After the presentation of this program, the various items were taken up separately and discussed in detail. A committee was appointed to consider the improvement of the statistics of the salmon fisheries and consisted of Dr. W. A. Clemens, chairman, N. B. Scofield, of the California Fish and Game Commission, and Dr. W. H. Rich. Another committee was appointed to consider the matter of adequate fishways over high dams. The members of this committee are N. B. Scofield, chairman, C. R. Pollock, supervisor of fisheries for the State of Washington, and Maj. J. A. Motherwell, chief inspec-

tor for British Columbia of the Department of Fisheries of the Dominion of Canada. In view of the recent development of a certain type of mechanical fishway, which has been advertised widely as having solved the problem of getting fish over high dams, the committee presented the following resolution, which was adopted by the executive committee:

Whereas constant and nation-wide propaganda has been maintained in newspapers, engineering and technical magazines, etc., conveying the impression, based on the partial success of the experimental fishway at the Baker River Dam, near Concrete, Wash., that a complete solution has been reached of the problem of safeguarding salmon runs jeopardized by the construction of dams; and

Whereas in the opinion of this federation no solution of this problem has yet been reached that can properly be considered as of general application; and

Whereas in the opinion of this federation the problem connected with each dam is individual and distinct; and

Whereas much of the work at Baker River Dam has so far been experimental and results there are not yet conclusive: Therefore, be it

Resolved, That the International Pacific Salmon Investigation Federation, at its meeting on December 2, 1926, strongly disapprove the propaganda mentioned as being unwarranted and misleading; and further be it

Resolved, That a copy of this resolution be presented to newspapers and periodicals and to such officials and others as may be interested.

ALASKA SALMON

The salmon investigations in Alaska have been continued under the direction of Dr. C. H. Gilbert, special assistant, and Dr. W. H. Rich, chief investigator of salmon fisheries. The tagging experiments in the channels of southeastern Alaska were continued, and a total of 13,100 salmon, representing four distinct species, were tagged and liberated. The results have not yet been compiled, but a report by Doctor Rich, covering the results of the tagging done in this district in 1924 and 1925, was published during the past year as Document No. 1005.

In addition to the tagging experiments conducted in the channels of southeastern Alaska, efforts were made, in cooperation with the fishery authorities of Canada, British Columbia, and the States of Washington, Oregon, and California, to tag fish caught in the ocean mainly by trolling. These fish are of two species—the chinook or king salmon and the silver salmon. Including the work done by all agencies, approximately 2,500 fish were tagged along the coast between Monterey Bay and the outside coast of southeastern Alaska. While the data are not yet sufficiently complete to warrant final conclusions, it appears quite certain that these fish range quite widely up and down the coast. The troll fishery for salmon thus assumes an interstate and international character, and this fact will have an important bearing on the nature of the methods adopted for the care of such of the salmon resources as are affected by trolling.

The intensive study and analysis (by means of scale examinations) of the salmon runs in a large number of the more important salmon streams of Alaska has been continued by Doctor Gilbert. Counting wiers for the enumeration of the spawning escapements have been maintained, as for a number of years past, in the Karluk and Chignik Rivers, at two of the streams that enter Olga Bay, Kodiak Island, at Thin Point and Morzhovoi Bay, Alaska Peninsula, and in

Egegik River, Bristol Bay. Plans are being made now for the construction of a counting weir in the Kvichak River, Bristol Bay. During 1926 particular interest attached to the run in the Karluk River, as this was the first return from a known escapement—that of 1921, the first year in which the counting weir was operated in this river. As the escapement of 1921 was one of the best on record (1,325,654 fish), a good run was expected and materialized. The total run, including the fish taken for commercial purposes and the escapement contained somewhat more than 4,500,000 fish. This indicates a return of approximately three fish for each one that escaped to the spawning grounds. Just how constant this productivity will be, remains to be determined by the returns in future years from known escapements; but it is a noteworthy accomplishment to have secured such data as these on a stream of the size and commercial importance of the Karluk. This intensive study of escapements and runs is considered to be of the utmost importance to the understanding of the factors that cause fluctuations in abundance in these great fisheries.

In addition to the detailed studies of the commercial catch and spawning escapement carried on by Doctor Gilbert at Karluk, a study of the history of the salmon in fresh water and of their seaward migration has been undertaken by Doctor Rich, assisted by Seymour P. Smith. In order to determine the number of young fish that migrate seaward, approximately 47,000 seaward migrants were marked during 1926, and the future runs of adult fish will be examined carefully for the return of marked fish. The total run of adults and the number of marked fish returning as adults will be determined, and from this it is believed that a fairly reliable measure of the total number of young salmon in the seaward migration each year can be determined with some accuracy. Such marking experiments will be continued in the Karluk River over a period of years in order to permit a study of the correlation between the size of the seaward migration and the future runs.

Considerable time was spent on Karluk Lake, which was mapped and sounded; and extensive observations of all spawning streams were made. The past year was one of exceedingly low water, and many of the spawning beds made early in the season were left exposed by the end of the summer. Further observations will be necessary to determine whether such conditions will affect the survival of the eggs. The study of such factors as this, which undoubtedly affect to a marked degree the survival of young salmon during their fresh-water life, is considered to be of prime importance, and plans have been made to continue these observations as a routine part of the intensive study of the Karluk River salmon.

SALMON OF THE COLUMBIA RIVER

The salmon-marking experiments conducted on the Columbia River by Harlan B. Holmes yielded greater returns during the season of 1926 than ever before. As in former years, the cooperation of the Oregon Fish Commission (especially in paying rewards for records of the recovery of marked fish) was responsible for a great part of the success of the work. The most pleasing returns obtained during the past season were from a lot of 50,000 yearling sockeyes, which

after their adipose and right ventral fins were removed, were liberated from the hatchery at Herman Creek in February, 1924. As a result of this marking, approximately 2,300 four-year-olds returned to the Columbia River and were taken in the commercial fishery. In addition, 96 of these fish succeeded in evading the commercial fishing gear and returned to the hatchery from which they were liberated, and their spawn was taken for artificial propagation. These 96 spawners produced a stock of eggs greater by one-half than that required to produce the fingerlings that were marked. Additional returns from this experiment are to be expected in 1927. Results such as these give promise of the possibility of maintaining commercially significant runs of salmon by artificial propagation.

A second experiment with sockeyes yielded 2,500 four-year-olds from 100,000 marked yearlings. In this experiment, however, none returned to the tributary in which the fingerlings were liberated. Why they did not return to their home tributary is an important question, to which there is no reliable answer as yet. This problem will be given special attention when future returns are being studied. The importance of this question lies in the fact that unless the fish return to the place of liberation the run can not be perpetuated by artificial propagation. A hatchery that is able to produce an entirely satisfactory return to the commercial fishery may fail in the long run because of inability to secure a breeding stock.

An experiment with chinook salmon conducted at the Big White Salmon River hatchery yielded 265 four-year-olds in 1926, bringing the total of returns from the experiment to 359. With the 5 and 6 year olds yet to return, this experiment gives promise of being the most successful of those with chinooks to date. As was pointed out a year ago, the greater returns from this experiment than from those in which fingerlings of the same class of fish were liberated at an older age, would indicate that the best time to liberate fingerlings of the fall run of chinooks is during the spring of their first year. More direct information on this important question is being sought by means of a series of experiments in which fingerlings from a single lot of eggs will be marked and liberated at various ages. A similar experiment is already under way with chinooks of the spring run. Comparison of the results of these two experiments should be enlightening, and it is hoped that they may point the way to more successful hatchery operations. For a more complete account of this work see p. 645.

Mr. Holmes also has continued an investigation of the blueback salmon of the Columbia River. The most important development during the past season was the observation that the seaward migrants from the Okanogan River spawning beds were so heavily infested with the encysted larvæ of tapeworms as to have their vitality greatly reduced thereby. An effort is being made to discover the alternate host of the tapeworm, in the hope that the infestation of the salmon may be reduced in the future.

HERRING OF ALASKA

During the season of 1926, George A. Rounsefell continued the herring investigation in Alaska, confining his activities to Prince William Sound, Cook Inlet, and Shuyak Straits. Preserved samples

also were obtained from the Shumagin Islands and Golovin Bay. Study of the racial characters of the herring has disclosed the existence of local races, which knowledge will be of great value in drafting regulations, as it indicates the relative independence of the fishing areas. Weights of herring are being tabulated in an effort to determine when the herring in each locality reach a condition suitable for packing.

The present data, although not conclusive, indicated the presence of dominant year groups, which may be one explanation for the great fluctuations in abundance. Further study may show the possibility of foretelling such periods. The investigation is discussed at greater length on p. 650 of this report.

During 1927 it is planned to continue the investigation, commencing the field work in April with some experimental tagging and continuing with the collection of samples in one locality throughout the season.

FISHERIES OF INTERIOR WATERS

COREGONINÆ OF THE GREAT LAKES

Dr. Walter Koelz has continued his investigations of the white fishes, and in addition to publishing Document No. 1001, "Fishing Industry of the Great Lakes," has completed the revision of the Coregoninæ, in which systematic relations and natural history of these fishes in the Great Lakes and Lake Nipigon are given in great detail, together with descriptions of several new species. This work, which is now ready for publication, embodies the most extensive and complete study of coregonine fishes in North America that has ever been made and lays the groundwork for an understanding of fishery conditions in the Great Lakes as a basis for protecting these rapidly declining fisheries.

The investigations on the herring of Lake Huron by Dr. John Van Oosten, described in the last annual report, were continued during 1926. The statistical data on the catch having been found wholly inadequate, biological data were sought which might furnish some clue to the trend of the intensity of fishing. That this fishing is very intense is suggested by the paucity of old fish in the commercial catches. Herring are known to reach an age of 11 years, yet extremely few individuals reach the sixth year of their life—the second year after most of them first join the schools of commercial sizes. The majority of the herring do not even reach the fifth year of life, and relatively few 3-year-old fish, many of which enter the commercial catches for the first time in the fall, escape the nets to return a year later as 4-year-old fish.

The biological data indicate, by the shifting in the age composition of the commercial catches, that the intensity of the fishing has steadily increased in recent years. In 1921 the 4-year-old fish were more numerous than the 2-year-olds, but since 1921 the former have become progressively less and the latter progressively more abundant each successive year. So intense does commercial fishing appear to be that a year class is virtually wiped out during its year of abundance in the commercial catches. Although at present the herring

are wholly unprotected and appear to be abundant in Lake Huron, each year the fishery is being prosecuted with increased vigor, for as the abundance of the more desirable species of coregonines decreases, the demand for the herring increases. It is evident from the biological data, however, that in all probability the herring can not withstand further exploitation without disastrous results. The results of this investigation are given in greater detail on p. 662 of this report.

Whitefish material has been collected by Doctor Van Osten from Lake Huron every year since 1923. These data indicate that the whitefish taken by gill nets in summer are younger than those taken in the fall. The summer catch consists principally of 4-year-old fish, a large percentage of which is sexually immature though of legal size. The fall catch consists mostly of 6-year-old fish, the 7-year-old individuals being second in abundance. The youngest fish taken in these samples were in the fourth year of life, the oldest in the thirteenth. Most of the fish of a year class probably do not spawn until the sixth year of life. This result was unsuspected, as it is generally believed that the whitefish attains sexual maturity in its third year. There is some evidence, however, that this is true in the case of the Lake Erie whitefish.

SURVEY OF SANDUSKY BAY IN LAKE ERIE

In response to an appeal from the Izaak Walton League at Fremont, Ohio, a preliminary survey was made of conditions in the Sandusky Bay region, with special reference to the destructiveness of commercial nets (especially the seines) to game fish. All complaints were considered and investigated. It was concluded that the commercial fishermen of the Sandusky Bay region were not as destructive of fish life in general, or of strictly game fish in particular, as many sportsmen alleged. On the contrary, it appeared very probable that the seiners, by capturing carp and goldfish, were aiding in preserving conditions necessary for the existence of game fish and waterfowl.

BIOLOGICAL SURVEY OF THE UPPER MISSISSIPPI RIVER

In July, 1926, A. H. Wiebe was assigned to make a biological survey of the upper Mississippi River. The object of this survey was to determine if the pollution from the cities of Minneapolis and St. Paul is destroying life in the Mississippi River. The part of the river covered by this survey extends from just above Minneapolis to just above Winona, about 110 miles below St. Paul.

The field work was carried on during August and September. After the completion of the field work (September 30) the plankton and the bottom samples were moved to the University of Wisconsin, at Madison, for study. The samples were analyzed and a report was prepared.

The investigation showed that pollution from the Twin Cities is a factor in the destruction of life in the Mississippi River from Minneapolis down to Red Wing, at the head of Lake Pepin, as is shown in more detail on p. 669.

KEOKUK DAM AND THE FISHERIES OF THE UPPER MISSISSIPPI RIVER

Immediately after the dam was constructed across the Mississippi River between Keokuk, Iowa, and Hamilton, Ill., there was begun a study of the effect of this and other changes in the Mississippi River upon the fisheries above the dam. The investigation was under the direction of Dr. R. E. Coker, then director of the fisheries biological station at Fairport, Iowa. An investigation of this kind necessarily involved observations over a long period of time. The process of adjustment of living animals to the changing conditions of environment in a stream is so gradual that safe conclusions can be drawn only after years of study. It would have been desirable if the observations could have been continued year after year without interruption, but this was not possible. The problem has not been out of mind, however, and studies have been made from time to time and tentative reports have been prepared. The time now seems ripe to put into form for publication the considerable data secured in the investigation. Accordingly, during the past year Doctor Coker undertook to complete the report and to make additional observations in the field. Accompanied by H. L. Canfield, he visited the Mississippi River at various points between Lake City, Minn., and Canton, Mo. The additional data gathered this year are now being incorporated in the report, which is nearing completion.

WESTERN TROUTS AND OTHER COLLECTIONS

In addition to investigating the New England smelt fisheries, Dr. W. C. Kendall also was occupied intermittently in studying and classifying trout material contained in numerous hitherto unstudied or little studied collections of trout from the Western States and Alaska, which represent years of accumulation. Inasmuch as these specimens were collected before the regions from which they came were affected by extensive fish-cultural distribution of nonindigenous species, this study is expected to throw greatly needed light upon the relationship of western species, and particularly upon the much-mooted problem of the so-called rainbow and steelhead trouts.

The wide fish-cultural distribution of these species and the varied experience with them in the east and abroad have given rise to a number of questions concerning them, and from time to time inquiries have been received for information and opinion. An inquiry particularly difficult to answer was from Doctor Ehrenbaum, of Hamburg, Germany, who wished to ascertain the specific identity of rainbow trout imported into Germany. Doubtless the rainbow-trout eggs sent to Germany were from the same two or three different species that seem to compose the mixture that (at least in the past) has been distributed from the hatcheries under the one name, "rainbow trout."

During the summer, through the Bureau of Fisheries, five specimens of rainbow trout, which were the result of introduction, were received from H. L. Kelly, executive officer of the fish and game commission of the Territory of Hawaii, with the request that they be identified.

Concerning these trout, Doctor Kendall replied that according to his view there are two sorts of "rainbow trout." One sort comprises *Salmo shasta* and closely related species, which are supposed not to be habitually sea run. The other sort comprises the migratory fish that has received the name of "steelhead" (*S. gairdnerii*). This name originally signified the migratory fish only; but there are other forms, perhaps distinct species and perhaps nonmigratory, that are more closely related to the migratory steelhead than they are to *S. Shasta*. These also have been popularly regarded as "rainbow trout."

Doctor Kendall regards the Hawaiian trout as the steelhead type of trout, but that does not signify necessarily that they were the migratory form; that is to say, the eggs that produced these fish may not have been taken from migratory fish (at the time recognized as such), but from trout supposed to be rainbows. While they are fish that have entered largely into the bureau's fish-cultural rainbow-trout output, they are not of the *Salmo shasta* category. The fish in question may or may not have been from migratory stock or from fish that are occasionally sea run.

Doctor Kendall was unable to say what the proper technical name of this fish should be, but suggested that not much violence to the present taxonomic situation or any greater confusion than now exists would ensue if they should be called *Salmo irideus*.

Late in the season specimens of so-called steelhead trout were received for identification from Prof. J. R. Dymond, of the biological department of Toronto University, Toronto, Canada, which he had collected in British Columbia and Washington. A specimen from Kalama, Wash., appeared to differ somewhat from those from Vancouver Island and Prince Rupert, as it did also from other specimens from the Columbia River and from specimens collected by Doctor Rich in Naknek Lake, Alaska, with which it was compared.

During the year other collections and specimens were received for study or identification. Particular mention should be made of the collection of Greenland chars made by Dr. Walter Koelz on the McMillan expedition in 1925. Also, a collection of salmonids, mostly chars, from Labrador, was delivered to Doctor Kendall in November by Columbus Iselin, a Harvard student, who conducted an oceanographic expedition, sponsored by Dr. Henry B. Bigelow, along that coast. The collection, although not large, was representative and well preserved, and the observations recorded in accompanying notes added much to the knowledge of conditions in that region, as pertain to the Salmonidæ.

A few specimens of salmon (*Salmo salar*) and recorded observations confirm the statement made by Abe Bromfield, McMillan's Eskimo interpreter, that there is no commercial salmon fishery beyond about 50° north latitude, but that in one or two places farther north one or more individuals occasionally are taken in cod traps. However, Bromfield asserted that a salmon fishery is carried on in Ungava Bay, concerning previous reports to that effect there has been some doubt.

The chars collected comprised two species—one that has been regarded as *Salvelinus fontinalis* and the other as *S. stagnalis*. The

first occurred only in lower Labrador, while the second was found along the entire coast. Both are sea run.

INVESTIGATIONS PERTAINING TO FISH-CULTURAL OPERATIONS

TROUT CULTURE

Feeding experiments with various diets and with several species of trout were continued during the summer by Dr. H. S. Davis and M. C. James at the Holden (Vt.) experimental hatchery. A more detailed account of the work at the station may be found on p. 675.

A comparison of three common foods—beef heart, beef liver, and sheep liver—indicated clearly the superiority of beef liver when trout are to be reared to a larger size than the small fingerlings commonly used for stocking waters. Apparently any one of the three foods is satisfactory with steelhead, rainbow, and brook trout when the fish are to be planted early in the season. However, when brook trout were held to the age of 6 months, beef liver was found to be markedly superior as a food and reduced the mortality approximately one-half and produced twice the growth of its nearest competitor—sheep liver.

A comparison of cooked sheep liver with the raw liver as ordinarily fed yielded conflicting results, probably due to a number of complicating factors. With brook trout the superiority of raw liver became more marked as the experiment progressed, and at the end of 103 days the total mortality among the fish fed cooked liver was four times as great as among those on a raw diet.

With young rainbow trout cooked liver showed a superiority over the raw product over a period of 94 days, both as regards mortality and growth. Inasmuch as many commercial growers have reported favorable results from the use of cooked food, it is planned to continue experiments along these lines.

Nothing has been found that offers any prospect of entirely replacing the fresh-meat diets. However, three substances were tried, which yielded very encouraging results as substitutes for part of the meat, and in mixture may even be superior to the pure-meat diets. These are soy-bean oil meal, a dried shrimp product, and fresh-water mussel meal.

Soy-bean oil meal is manufactured from the residue after the oil has been expressed from the bean. An important property is the high percentage of protein, which more closely resembles animal protein in structure than do proteins from vegetable sources. Experiments with this meal were unsuccessful at first, but after adjusting the quantity to a basis of 50 per cent meal and 50 per cent meat, both brook and lake trout fingerlings were maintained for several months with nominal loss, although the growth did not quite equal that of the controls. Further experiments will be undertaken on a larger scale, but it is believed that this material will be most useful as a food for yearlings and adult fish.

The waste from shrimp factories, under the name of "shrimp bran" and "shrimp meal," frequently have been used in trout diets with more or less beneficial results. These products usually contain a large percentage of chitinous shell, which is of little value except

as roughage and is entirely unsuited to feeding small fish. The dried shrimp used at Holden is a special grade, consisting of the abdomen only, which has been freed from the surrounding shell. After being soaked in water for several hours, it can be ground sufficiently fine to serve as food for fish 2 to 3 inches long and upwards. When used alone it is inadequate, as shown by a sharp increase in the mortality; but the addition of sheep liver quickly brought the mortality back to normal. A continuation of the experiment on the basis of 50 per cent shrimp and 50 per cent sheep liver resulted in practically no losses and an exceptionally rapid growth.

The results with "clam" meal were particularly encouraging, although, owing to the small amount available, it was possible to try it on a small scale only. A lot of steelhead fingerlings fed beef heart with 25 per cent "clam" meal over a period of 97 days showed a mortality less than one-half as great and a growth over one-fourth greater than the controls fed beef liver. The meal is made from the dried "meats" of fresh-water mussels and is a by-product of the mussel-shell industry. Inasmuch as it is impossible to obtain a satisfactory grade of this meal in quantity, an attempt will be made to manufacture it at the Fairport (Iowa) biological station.

A series of experiments to determine the effect of sunlight on young trout yielded very interesting results. Various lots of fish from the same source were placed outdoors in different compartments of the same trough, with some compartments covered so as to exclude all direct light, others were covered with ordinary window glass, which would absorb the ultra-violet rays, while some compartments were exposed entirely. All lots were fed alike, and the flow of spring water was abundant, so that on the hottest summer days there was no appreciable difference in temperature between head and foot compartments of the same trough. The only difference in the various lots was the rather abundant algal growth in the exposed compartment, but it is not thought that this would react to the disadvantage of the fish.

Lake trout in advanced fry and fingerling stages showed losses of 18.4 per cent in the covered lot and 33.7 per cent in the exposed lot. A repetition of the experiment, using advanced fry and fingerlings of rainbow trout, gave even more striking results. In this case, over a period of about two months, the protected fish suffered a loss of only 1.2 per cent; in the glass-covered group the loss was 3.5 per cent; while among the fish that were entirely uncovered there was a mortality of 38.1 per cent. No record of the weights of the lots was taken, but plainly the condition of the fish was correlated with the mortality, inasmuch as the groups exposed to sunlight, which suffered the greatest loss, showed poor condition and comparatively slow growth.

On the face of the results it would appear that ultra-violet light is injurious to trout, at least in the younger stages, and that the common practice of protecting trout at the hatcheries from direct sunlight is based on sound reasoning. It should be pointed out, however, that the fish were held in shallow troughs and that the results may not be applicable to fish held in ponds or raceways. Further experiments on the influence of light are planned for the near future.

The initial steps in an extensive program of selective breeding of brook trout have been taken at the Holden station. By a rigid selection of the parent stock it is planned to develop strains in which desirable characters will become fixed and thus create a brood stock that will exhibit superior qualifications with regard to disease resistance, rapid growth, early maturity, prolific egg production, and greater vigor. During the past season, eggs were taken from fish selected for early maturity and rapid growth and also from fish characterized by desirable form and color. Fingerlings that show great resistance to disease and rapid growth have been segregated for future breeding purposes.

It is evident that practical results from these experiments can not be expected for some time, but it is believed that in a few generations it will be possible to develop a strain of brook trout much better adapted to hatchery requirements than the stock now available.

A necessary preliminary of the work in selective breeding is the expansion of the capacity of the Holden station. The construction of trout ponds has been continued during the past year, so that there are now in use eight rearing ponds, one small pond for brood stock, and several hundred feet of raceways. There is opportunity for the further extension of the pond system, and it is essential that a number of ponds and raceways be constructed in the near future if the progress of the investigations is not to be hampered seriously.

The abundance of good trout streams in the vicinity of the Holden hatchery has afforded an opportunity for the inauguration of much needed field work on certain ecological aspects of fish planting. It is planned to extend the scope of this work considerably during 1927. Cooperation has been maintained with the Vermont Department of Fish and Game in investigations of hatchery mortalities and a survey of waters for the purpose of formulating a stocking program.

POND CULTURE

The investigation of various problems relating to the propagation and rearing of pondfishes was inaugurated at the Fairport (Iowa) biological station during the summer of 1926 by Dr. H. S. Davis, assisted by Russell F. Lord and the station force. This is a field that has received very little attention in this country, although pond culture has reached a comparatively high stage of development in Europe. Details of the work are given on p. 678.

During the past summer 21 ponds were utilized in these investigations, which, although only preliminary in nature, already have yielded valuable results. The experiments were confined almost entirely to problems connected with the propagation of the large-mouth bass and bluegill sunfish, but it is planned to broaden the scope of the work in the future so as to include other species of pondfishes.

One of the problems of fundamental importance in pond culture is the proper treatment of the ponds to produce the maximum amount of food. Questions arise as to the quantity and type of rooted vegetation, the advisability of wintering the ponds wet or dry, the effect on the rooted vegetation and plankton of cultivating the bottom and treating with lime. These and many other problems of a similar nature are receiving consideration in the experiments at Fairport.

Another line of investigation concerns the use of fertilizers as a means of increasing the basic food supply.

In rearing bass and other predaceous fishes, the use of forage fishes affords a promising line of investigation. The introduction of forage fishes is designed to furnish a supply of small fish that will serve as food for the bass. This not only will tend to prevent cannibalism, but as these fishes feed on plankton and plant materials will greatly increase the amount of food available to the bass.

During the past season three species of forage fishes—viz, goldfish, golden shiners (*Notemigonus crysoleucas*), and black-head minnows (*Pimephales promelas*)—were introduced into the bass ponds with excellent results. The black-head minnow promises to be especially desirable for the purpose, as it multiplies rapidly, continuing to spawn throughout the spring and early summer, so that a supply of small fish is always available. Furthermore, they are primarily bottom feeders, and consequently there is little direct competition with the young bass for food. Obviously, the use of forage fishes introduces a number of problems that are in urgent need of investigation. These include such questions as what is the most desirable species of forage fish to use with different game fishes, the advisability of introducing two or more species of these fish in the same pond, and the proper methods of handling them to obtain maximum results.

Other problems in pond culture that are being investigated relate to the number of brood fish required per unit area; the total yield of fish, both as to numbers and weight, that can reasonably be expected; and the advisability of rearing two or more species of food fishes in the same pond.

As in the case of trout an attempt is being made to develop superior strains of pondfish by selective breeding, special emphasis being placed on rapid growth.

PATHOLOGY OF FISHES

As in previous years, special attention has been paid by the pathologist, Dr. H. S. Davis, to a study of the diseases that are causing serious losses at the trout hatcheries. The Holden experimental hatchery affords exceptionally favorable conditions for such investigations, for here the fish are under close observation at all times and any outbreak of disease can be discovered in its early stages. During the summer of 1926, in addition to octomitiasis, which is now well under control, two infectious diseases appeared among the fingerling trout and caused considerable losses before control measures could be developed.

The most serious losses were caused by an infection of the gills, which had not previously been recognized. The disease appeared during June and July among fingerlings of brook, rainbow, steelhead, and black-spotted trout, and also among some small landlocked-salmon fingerlings. It is due to an infection with bacteria, which form a luxuriant growth over the surface of the gills. They occur as long, threadlike filaments, which usually lie side by side so as to form a more or less continuous layer over the surface of the epithelium. The bacteria are colorless, transparent, and very difficult to distinguish, even under high magnification. They are most

abundant on the outer third of the gill filaments, where, evidently as the result of an irritation caused by their presence, there is a rapid proliferation of epithelial cells. This causes the free ends of the gill filaments to become greatly enlarged and in some cases distinctly club shaped. One of the most striking features of the disease is the fact that as a result of the rapid growth of the epithelium, the gill filaments often become fused, especially near the tips; and in extreme cases all the filaments of each gill may become united into a continuous mass.

In addition to the marked proliferation of epithelial cells, there is a greatly increased secretion of mucus over the gills, in which particles of sand and débris become entangled. Consequently, the gills present a very characteristic appearance, which furnishes the only reliable means of diagnosing the disease. In other respects the fish exhibit no characteristic symptoms, and in fact appear virtually normal until a very short time before death.

Fortunately the disease is controlled easily, one or two treatments with a solution of copper sulphate being all that is required. The fish were placed in a 1:2,000 solution of copper sulphate for one minute and then removed at once to running water. Only a few of the weakest fish were injured by the treatment, while nearly all the bacteria were destroyed. When followed by a second treatment the following day, the bacteria entirely disappeared and there was no recurrence of the disease.

An infection of the fins appeared in two instances among fish that were being held in hatchery troughs, but the disease did not spread widely. The disease first appeared in a trough of steelhead fingerlings but was quickly brought under control by a treatment with copper sulphate. Later in the season the same disease broke out in a trough of small rainbow fingerlings. In all probability it is a bacterial infection, but owing to the fact that several species of bacteria were always present on infected fins, it has not yet been possible to determine definitely which was the cause of the disease.

In most instances the infection is first noticeable on the pectoral fins, which become thickened and opaque, but later all the fins may become involved. A microscopical examination shows that infection first occurs on the outer margin of the fin and appears as a whitish discoloration. This is due to a thickening of the epithelium, which gradually extends toward the base of the fin. Later, the thickened region disintegrates and the fin rays become frayed and broken. Eventually the fins may be destroyed entirely, although in the majority of cases death intervenes before this occurs. The disease can be controlled by the copper-sulphate treatment, as used in the case of the gill disease.

"Fin trouble" is quite common in many hatcheries, and while it is not always due to the same cause, it is believed that in many cases it is the same as that which occurred at Holden.

During the spring of 1926 there were very heavy losses among the advanced brook-trout fry at the Holden station. This mortality occurred shortly after the fish began to feed, and in one lot the loss was virtually 100 per cent. This heavy mortality was confined to fish hatched from eggs that had been held in brook water or a mixture of brook and spring water. Fish hatched from eggs from the same

source, but which had been held in spring water, suffered a mortality of only about 10 per cent. The cause of the excessive losses among these fish has not been determined definitely. Some of the loss undoubtedly was due to an infection with the protozoan parasite, *Octomitus salmonis*, and there is little doubt but that the mortality among the fish in spring water was due almost entirely to this parasite. However, another factor appears to have been involved in the heavy mortality among the fish in brook water. It is possible that the fish were weakened as a result of retarded development caused by the low temperature of the brook water. It is interesting to find that a similar mortality has occurred annually in several State hatcheries where the eggs are held in cold water to retard their development. In fact, the information at hand indicates that the common practice of holding eggs at a temperature just above freezing is objectionable and often results in greatly lowered vitality.

Octomitiasis has been prevalent at Holden for a number of years, and the losses from this disease were so large that all attempts to hold fingerling trout through the summer had been abandoned. However, during the past two summers, since the station has been used as an experimental hatchery, fingerlings have been carried through the summer with comparatively little loss from this disease. It has been found that while it is virtually impossible to get rid of Octomitus entirely, the severity of the infection can be reduced greatly by rearing the fingerlings in ponds, where they have more room than in the hatchery troughs and can obtain some natural food, such as insects and Entomostraca. In several instances, fish that had become heavily infected with Octomitus in the hatchery, when removed to ponds showed marked improvement within a week or ten days; and at the end of three or four weeks the parasites had disappeared very largely, the mortality was low, and the fish were making rapid growth.

In addition to work at the bureau's stations, the pathologist, at the request of the State authorities, visited several State hatcheries to investigate the causes for heavy losses among fish at these stations and to suggest remedial measures. He was consulted by a number of goldfish breeders, also, regarding mortality among their fish.

In March, 1926, an investigation was made at the Washington laboratory of the mortality occurring at the Central Station aquarium following chlorination of the water. Dr. R. S. Taylor, of the division of fishery industries, studied the chemical aspects of the problem, while M. C. James made observations on the biological phases. It was found that trout were killed by the concentrations found in the ordinary chlorine dosages (0.3 p. p. m.) of municipal water supplies. Survival in spite of such chlorination generally is due to a reduction in the concentration through interaction with organic matter and aeration.

A number of chemicals, all reducing agents, were found to be effective in eliminating all chlorine in an aquarium supply and at the same time were harmless to fish. Among these, sodium bisulphite and sodium thiosulphite were considered most satisfactory. A 1:1,000,000 concentration of the former is sufficient to neutralize the chlorine usually present in municipal waters, while sufficient thiosulphite must be introduced to give two or three parts per million.

Observations on the pathology of chlorine poisoning indicate that extremely small quantities have the same general effect on fish that much greater amounts have on higher animals. Death is the result of suffocation induced by inhibition of the respiratory action. Whether this is due to constriction of the arterioles or to edema at this point is uncertain. The most striking symptom in gassed fish is the congestion of the visceral blood vessels. It is noteworthy that after exposure to chlorine for any length of time fish will not recover when removed to chlorine-free water.

INVESTIGATIONS IN WISCONSIN LAKES

During July and August, 1926, limnological studies were continued on the lakes of northeastern Wisconsin by the Geological and Natural History Survey of that State in cooperation with the Bureau of Fisheries. Observations were made on 73 lakes, most of them situated in Vilas County. Forty-six of these were visited in 1925, but 27 were visited for the first time in 1926.

Biological and chemical laboratories were established in two buildings at the State forestry headquarters at Trout Lake, Wis. Two biologists, Dr. E. A. Birge and Prof. C. Juday, and two chemists, Loren C. Hurd and Rex J. Robinson, were engaged in this investigation.

In size, the various bodies of water ranged from a minimum of an acre or two to a maximum of 1,500 acres. The depths varied from 2 meters to 35 meters; in most of them the maximum depth does not exceed 15 meters.

The lowest surface temperature noted was 18° C., and the highest was 23.5°. The temperature of the bottom water in the deeper lakes varied from 4.7° C. to 10°, but the temperature of the bottom water in the shallow lakes was substantially the same as that of the surface.

A complete set of chemical and biological determinations on a sample of water comprised 14 different items, not including a reading of the temperature of the water at the time the sample was taken. A single series of samples, extending from surface to bottom, in Trout Lake, for example, included 83 different determinations in addition to 14 temperature readings taken at the time the samples were obtained. Field methods have been developed that make it possible to complete such a set of determinations in one day.

These lakes have relatively soft water; the fixed carbon dioxide ranges from a minimum of less than 1 cubic centimeter per liter of water to a maximum of a little more than 15 cubic centimeters. In 25 of these bodies of water the fixed carbon dioxide was less than 2 cubic centimeters per liter of water, but most of the others had between 5 and 10 cubic centimeters. The bottom water of some of these lakes possessed a distinctly larger amount of fixed carbon dioxide than the surface water; in Wild Cat Lake, for example, the surface water possessed 15.4 cubic centimeters per liter and the bottom water (11 meters) 22.3 cubic centimeters on August 24, 1926.

The lowest readings for hydrogen-ion concentration were obtained in the lakes having the softest water; that is, in those having less than 2 cubic centimeters of fixed carbon dioxide per liter of water. In such lakes the reaction varied from pH 5.2 to pH 6.5. In the lakes having a larger amount of fixed carbon dioxide the range was

from pH 7 to pH 9.2. In the deeper lakes the bottom water usually gave lower readings than the surface water; in Trout Lake, for example, the range was from pH 7.6 at the surface to pH 6.6 at the bottom (32 meters).

The surface stratum in these lakes was well supplied with dissolved oxygen; the amount varied from 4 cubic centimeters to a little more than 7 cubic centimeters per liter. In some of the deeper lakes the lower water possessed very little or no dissolved oxygen at all.

The quantity of free ammonia in the surface water ranged from a minimum of 0.016 to a maximum of 0.68 milligram per liter of water. In lakes having a depth of 5 meters or more, the lower water usually contained a larger amount of free ammonia than the upper. In Lake Mary, on July 12, 1926, the surface water yielded 0.024 milligram of free ammonia per liter, and the bottom water (20 meters) 2.40 milligrams, or a hundred times as much as the surface. In most instances, however, the bottom water yielded not more than 5 to 10 times as much as the surface.

The amount of combined or organic nitrogen in the surface water varied from a minimum of 0.073 milligram per liter to a maximum of 0.88 milligram. The quantity of organic nitrogen depends chiefly upon the amount of plankton that is present. In some of the deeper lakes a larger amount of organic nitrogen was found in the lower than in the upper water, but the reverse was true in the majority of these lakes.

Most of the lakes contained no nitrite nitrogen, or only a trace; a few yielded measurable amounts of nitrite, the amount varying from 0.001 to 0.01 milligram per liter. Similar results were obtained for the nitrate nitrogen, but as much as 0.08 milligram per liter was noted in the bottom water of Trout Lake on August 14. In only a few instances did the nitrate nitrogen exceed 0.02 milligram per liter, and only a trace or none at all was found in the great majority of the samples.

The 1926 observations included the organic phosphorus as well as the soluble; only the latter was determined in 1925. No soluble phosphorus was found in the upper water of one lake and only a trace in another; in all of the other lakes the amount in the upper water varied from a minimum of 0.003 to a maximum of 0.015 milligram per liter. In many instances the soluble phosphorus was uniformly distributed from surface to bottom, but in others there was a more or less marked increase in the lower water. In Lake Mary there was no soluble phosphorus at the surface and at 3 meters, but 0.75 milligram per liter of water at 20 meters on July 12, 1926.

The quantity of organic phosphorus in the upper water varied from a minimum of 0.01 to a maximum of 0.05 milligram per liter, but it exceeded 0.04 milligram in only three lakes. In most of the lakes the upper water contained from two to four times as much organic phosphorus as soluble phosphorus. In some of the lakes the organic phosphorus was substantially the same from surface to bottom, but in others the lower water contained from two to four times as much as the upper.

The silica varied from only a trace or none at all to a maximum of 10 milligrams per liter in one lake. In most instances, however,

the amount did not exceed 5 milligrams per liter, especially in the upper water. In some lakes there was a marked increase of silica in the lower water; in Trout Lake the surface water contained 3 milligrams and the bottom water (30 meters) 8 milligrams per liter on July 9, 1926.

The chlorides varied from 0.53 to 3.1 milligrams per liter of water; in most instances the amount was between 1 and 2 milligrams per liter.

Two to 10 liter samples of centrifuged water were evaporated for the purpose of obtaining the residue. These residues varied in amount from 12 milligrams to 88 milligrams per liter of water in the upper stratum. A maximum of 144 milligrams per liter was obtained in the lower water of one lake. In this connection it may be noted that the hard-water lakes of southeastern Wisconsin yield from 165 to 255 milligrams of residue per liter. These residues are now being used for the determination of the quantity of organic carbon, and quantitative determinations of other substances therein are also contemplated.

A Foerst electric centrifuge was used for the purpose of making a quantitative study of the plankton. This material was dried in an oven, weighed, ashed in an electric furnace, and then weighed a second time in order to ascertain how much organic matter it contained. The quantity of this organic matter varied from a minimum of 265 milligrams per cubic meter of water to a maximum of 10,875 milligrams. In most of the lakes, however, the amount ranged from 800 to 2,000 milligrams per cubic meter.

SHELLFISH AND TERRAPIN

OYSTERS

Owing to the increased appropriations for the oyster investigations, the program of work relating to this important fishery was considerably extended under the supervision of Dr. P. S. Galtsoff. The investigations made during the fiscal year consisted in (1) surveying the natural oyster beds and reefs, (2) study of the spawning of the oyster, (3) studies of the behavior of the oyster larvæ and of the conditions controlling setting in northern waters, (4) experiments in oyster-seed production and collection, and (5) study of the oyster drill.

SURVEYS OF THE NATURAL OYSTER REEFS AND BEDS

Texas.—In compliance with the request of the game, fish, and oyster commissioner of Texas, a survey of coastal waters was made by Dr. P. S. Galtsoff during February and March, 1926. The purpose of the survey was to determine what practical measures should be adopted in order to prevent further depletion of the natural reefs and to maintain, or if possible to increase, the production of oysters in the State.

The survey covered the region extending 170 miles along the coast of the Gulf of Mexico, from Corpus Christi to Galveston. Field observations were made in cooperation with the State authorities, who assigned the State boat *Pearl* to the investigator for this pur-

pose. The *Pearl* covered 1,034 miles between Corpus Christi and Galveston, visiting the following localities: Corpus Christi and Nueces Bays, Corpus Christi Pass, Aransas Pass, Aransas Copano, Mesquite, San Antonio, Espiritu Santo, Matagorda, Lavaca, Karankawa, and Trespalacios Bays, Cedar Lakes, and West, Galveston, and East Bays.

The program of observations consisted in the examination of the bottoms, readings of temperature, determination of salinity, alkalinity, and turbidity of water; measurements of currents; examination of oyster reefs; and studies of the plankton.

It was found that oyster reefs in Texas coastal waters produce enormous quantities of oysters, some of which have little market value; on the other hand, bottoms suitable for oyster culture are rather scarce. It is necessary, therefore, to exploit the suitable bottoms to their full capacity by planting young oysters on them, and to use the overcrowded reefs as the source of an almost unlimited supply of seed oysters.

The following recommendations for the development of the oyster industry were based on this survey:

1. It is recommended that the State authorities encourage the development of oyster farming under private enterprise and discourage the exploitation of the natural beds as rapidly as the development of oyster farming will permit.

2. In the meantime, it is recommended that the State authorities aid in demonstrating the practicability of oyster farming and in increasing the production of the present natural beds by (*a*) the planting of single or culled young oysters over the bottoms in Aransas Bay, Mesquite Bay, Lavaca Bay north of Sandy Point, Kellers Bay, Karankawa Reef, mouth of Trespalacios Bay, and Matagorda Bay, between Portsmouth and Pallacios Points; (*b*) the planting of shells in Nueces Bay; (*c*) experimental planting of shells on Karankawa Reef and in other bays in order to determine the setting areas.

3. Certain biological data should be collected, as, for example, (*a*) observations of the time of spawning in various bays; (*b*) continuation of the taking of samples of water for further examination in the laboratory of the United States Bureau of Fisheries.

4. Where reefs form barriers, preventing the mingling of fresh water and sea water, it is suggested that the State encourage the cutting of passes through the reefs by permitting the use of the "mud shells" for commercial purposes.

South Carolina.—A survey was made in April, 1926, by Dr. P. S. Galtsoff and H. F. Prytherch, of the coastal waters of South Carolina for the purpose of determining the most suitable methods for conserving and building up the oyster industry. Headquarters were established at Beaufort, and an examination was made of the coastal region from Cape Romain to the Savannah River so as to cover the most important and representative oyster-growing localities. Determinations were made of the physical and biological characteristics of the oyster-growing regions as a basis for recommending such experimental planting operations and methods of culture as will bring about the greatest development of the oyster fishery and the most successful utilization of the oyster grounds. A report covering the results of the investigation has been published, together with a chart

of the coast showing the general location and extent of the natural oyster beds and the distribution of salinity.

It was found that, with very few exceptions, the natural oyster beds are situated along the shores on the tidal flats and lie in a zone between low and high water mark. This is due largely to the fact that setting occurs, for the most part, in the zone between tide marks. There are areas of the bottom, however, below low-water mark in the tidal streams, sufficiently firm and unshifting, which can be utilized for growing oysters, which can be obtained as seed by planting brush and shells in the vicinity of the natural beds.

High-grade single oysters can be cultivated on these bottoms where setting does not occur, but their marketable quality will depend upon the environmental conditions in each locality and the care expended on the beds. Some of the natural oyster beds are depleted and should be restored and enlarged by a more extensive planting of shells.

The following recommendations are offered in order to restore the natural wealth of oyster resources, increase the production, and improve the quality of the oysters grown in South Carolina waters:

1. A greater quantity of shells should be returned to the natural beds.
2. The natural bed should be extended by planting shells on adjacent firm bottoms.
3. Depleted oyster beds should be restored by planting seed and adult oysters.
4. The closing of depleted areas until they are built up to a self-maintaining basis is advised.
5. The collection of set on brush and shells planted on tidal flats should be practiced.
6. Seed oysters should be transplanted to suitable bottoms below low-water mark.
7. The experimental transplanting of seed oysters on the tidal flats in the upper portions of the streams where setting does not occur should be undertaken.
8. Adult oysters should be transplanted when necessary to prevent overcrowding and to facilitate growth and fattening.
9. The development of oyster farming should be encouraged by leasing the grounds and protecting the private beds.

Massachusetts.—During the latter part of the summer a survey of important oyster grounds on Cape Cod was made by Dr. P. S. Galtsoff and H. R. Seiwell. The survey covered the following localities: Wareham River, on the Buzzards Bay side of the cape, and Waquoit, Cotuit, Centerville River, and Chatham on the ocean side. The first locality differs from the others in being chiefly an important seed-producing region, while most of the oyster bottoms on the ocean side of the cape can be regarded as growing grounds primarily. This especially refers to the Cotuit region (Osterville Harbor), where not a single young oyster can be found in spite of a very careful examination of the shores and bottoms made in August. It is difficult to tell why the set does not occur on this bay, where general conditions (namely, the character of the bottom, the temperature and salinity of the water, and the tidal currents) are favorable. The probable causes may be either the failure of oysters to spawn or the

death of the oyster larvæ during their free-swimming period of life. It has been noticed that adult oysters were rather scarce, being scattered over a large area of bottom, a condition that, according to the experiments described later, should be regarded as unfavorable for successful spawning.

Waquoit Bay.—The oyster business in Waquoit Bay is of no importance at present. The main reason for the decline is the accumulation of eelgrass on the bottom, where it decays, giving off hydrogen sulphide in great abundance. A region free from eelgrass, and which appears to be excellent for oyster planting, was found at the mouth of Quostinet River. The salinity in this section of the bay ranges from 17 to 18 parts per thousand.

Chatham region.—Conditions similar to those found in Osterville Harbor were found in Oyster River, Oyster Pond, Mill Pond, and Stage Harbor. Observations made in this region indicate that setting occurs irregularly and is never abundant. The grounds, however, are excellent for growing oysters for market. The salinity in Oyster Pond and Mill Pond averages 30 parts per thousand.

Centerville region.—The natural oyster beds of this section, located in Bumps and Chequaquette Rivers, have been so depleted in the past 15 years that no oyster business is carried on there at present. A survey failed to show any mature oysters on the beds; consequently there were no seed oysters. Only a few small oysters, probably 2 or 3 years old, lying in clusters, were discovered on the bottom at the mouth of Bumps River. As no attempt at oyster culture has been made for about 12 years, the continual taking up of the mature oysters has exhausted the beds. The survey shows that the natural oyster beds can be developed again by planting mature oysters for spawning on the old oyster bed in Bumps River and the former value of the region as seed-producing ground thus be restored. This section is not suitable for raising marketable oysters, because in certain seasons of the year they turn yellow and acquire a disagreeable flavor, but it is very suitable for raising seed oysters to supply Hyannis, Chatham, and other farms nearby.

The results of the survey of Wareham River are discussed later in connection with the experiments on spat collectors.

Mississippi.—At the request of Hon. J. J. Kennedy, mayor of Biloxi, Miss., J. H. Weatherby, temporary investigator of the bureau, was detailed to make an investigation of the Mississippi Sound and adjacent waters. The observations were begun on October 15, and will be carried on throughout the year, covering various phases of the life history of the oyster. The purpose of the investigation is to determine the present conditions of the natural reefs and to work out the method by which the oyster crop in the State can be increased. The bill passed by the Louisiana Legislature, forbidding the citizens of the State of Mississippi to take oysters in Louisiana waters, and the fact that at present about 50 per cent of oysters canned in Mississippi are brought from Louisiana, make this investigation imperative. A preliminary survey of the reefs in the western section of Mississippi Sound was made in October by Dr. P. S. Galtsoff. Special attention was directed to the reefs near Pass Christian, where high mortality occurs rather regularly among the young oysters. Local oystermen attribute it either to the high salinity of water or to the destructive activity of conchs. The ob-

servations failed to support either view, as the salinity of the water was rather low and no conchs were found on the reefs.

Preliminary observations tend to show that the planting of shells on suitable bottoms will probably increase the production of oysters to such an extent that Mississippi consumers will be independent of Louisiana.

North Carolina.—A survey of Core and Pamlico Sounds has been undertaken in cooperation with the State fisheries commission, Mr. Seiwell, temporary investigator of the bureau, conducting the field work. Since 1920 over $300,000 has been spent by the State in planting oyster shells in these waters. It was the purpose of this investigation to ascertain the results of the planting operations; to determine the value of various sections of the sound as seed-producing and oyster-growing grounds; and to acquire fundamental knowledge regarding the physical, chemical, and biological conditions existing in these waters and controlling the growth and propagation of the oyster. Observations extending over a period of two months (November and December) indicate that in Pamlico and Core Sounds the currents, and consequently the distribution of the salinity, depend almost entirely upon the winds; that the fluctuations in the temperature of the surface water can be correlated directly with a rising or falling air temperature; that bottom temperatures are usually 2° to 3° C. lower than that of the surface; that the oxygen content of water varies from 1,000 milligrams per liter to 8.85, and free CO_2 varies from 2.2 to 6.8 milligrams per liter. Numerous determinations of Ca content in water were made, and many bottom and plankton samples were collected and analyzed. The work will be carried on throughout the year, and special attention will be directed to the study of spawning of oysters and abundance and distribution of set.

SPAWNING OF THE OYSTER

In the summer of 1926, experiments were conducted by Dr. P. S. Galtsoff at Woods Hole, Mass., on the spawning reactions of the oyster, which were studied under laboratory conditions. Both male and female oysters can be induced to spawn by raising the temperature of the water. However, at a constant temperature the female can be forced to spawn by adding sperm to the water. The details of this experiment are given at greater length on p. 653 of this report.

The following practical applications of the experiments can be made: 1. In certain localities the female oysters can be induced to spawn by adding sperm to the water.

2. For successful spawning, the oysters should be planted on the spawning grounds as densely as possible.

BEHAVIOR OF THE OYSTER LARVA AND CONDITIONS CONTROLLING SETTING IN NORTHERN WATERS

In addition to the practical experiments, studies were made in Milford Harbor by H. F. Prytherch in 1926 dealing primarily with the larval and subsequent attachment periods, both of which are of great importance from a scientific as well as practical standpoint.

Larval period.—In Milford Harbor oysters usually spawn between July 15 and August 1, the exact date varying somewhat each year. Then follows a light set about the 1st of August and a heavy set around the middle of the month. During the interval between spawning and setting the oyster larvæ supposedly swim about in the harbor. A large number of plankton collections were made, therefore, to determine their abundance, distribution, and growth. However, on examining these collections, made at various depths and different stages of the tide, it was found (1) that very few oyster larvæ were swimming about; (2) that of the few larvæ collected the majority were either a day or two old or were about 10 days old and nearly ready to set; (3) that when the tide was at low slack water the larvæ were most abundant; and (4) that the total number of larvæ found in daily collections, extending over a month, scarcely reached 100, while millions of them were found attached in the same place later.

In a series of bottom samples taken in the vicinity of the spawning beds many larvæ in the intermediate and late stages of development were found. This is but natural, for the oyster larvæ are adapted to either a free-swimming or a bottom existence.

These studies revealed the fact that the strong tidal currents cause the larvæ to settle to the bottom, thus avoiding their being carried out of the harbor. In other bodies of water, such as Great South Bay, Long Island, where the tidal currents are insignificant, the oyster larvæ were found to swim actively throughout the larval period. This investigation shows that the oyster larvæ, by settling to the bottom, are able to remain and set in the vicinity of the spawning beds, affording a sound basis for the development of inshore areas for the production of seed oysters.

Attachment or setting of oyster larvæ.—The location and distribution of the oyster beds in any locality is determined largely by the zone in which the oyster larvæ attach. This zone varies considerably in different regions, as, for example, in Long Island Sound and Milford Harbor setting occurs from the bottom to a point about 2 feet above low-water mark; in Great South Bay, Long Island, it occurs from the bottom to nearly high-water mark; while in South Carolina setting occurs between low and high water marks in the lower portions of a stream and only below low-water mark in the adjacent upper portions. Attempts to attribute the possible causes of this phenomenon directly to distribution of salinity, temperature, food content, and amount of sediment in the water have failed. In Milford Harbor, by means of floating and stationary spat collectors, tide gauge, and current meter, it has been found that the predominating factor controlling and limiting setting is the velocity of the tidal current; that heaviest setting occur during the period of low slack water, and continues as the tide begins to run flood, gradually becoming less intense as the velocity of the current increases, and finally ceases altogether when the current attains a velocity of approximately 10 centimeters, or one-third foot, per second. The upper limit of setting varies somewhat according to the tidal conditions, and especially the difference in levels of slack water at the time when setting occurs each year.

Drift-bottle experiments in Long Island Sound.—On September 18 to 21, 1926, 500 drift bottles, with drags attached, were released off

Stratford Point and Milford, Conn., by Mr. Prytherch. Up to the present time over 50 per cent have been recovered. The general drift of the water along the Connecticut coast from Bridgeport to New London is in an east-northeast direction. This shoreward movement of the water is due to a flood tide current that rotates gradually in a clockwise direction, so that by the last of flood it is running in a northeasterly direction.

These observations, together with studies of the distribution of the oyster larvæ, indicate (1) that a set on the offshore beds in Long Island Sound is obtained from oysters in their vicinity, and not from the inshore beds or those found in the bays and harbors; (2) in oyster-cultural operations on the offshore grounds the spawning beds should be located just seaward and southwest of the areas planted with shells.

OYSTER-SEED PRODUCTION AND COLLECTION

The studies and experiments made in 1925 by Mr. Prytherch in Milford Harbor, Conn., showed that such inshore bodies of water can be developed as oyster-seed producing areas by establishing spawning beds and planting suitable set collectors on the tidal flats. Several kinds of collectors were used successfully in obtaining a set, and of these, wirebaskets filled with oyster shells[3] proved to be the cheapest and most practical type. It was evident that the shape of the basket should be changed so as to enable the oyster larvæ to penetrate more easily and attach on the shells in the center. For the experiments in 1926 less expensive shell containers were constructed of spruce lath, triangular in shape, and each had a capacity of 2 bushels and covered an area of 2 square feet. The oyster shells used averaged 250 to the bushel.

At Milford, Conn., and Wareham River and Wellfleet, Mass., the lath crates were tested for the collection of seed oysters. A résumé of the experiments in each locality is as follows:

Milford, Conn.—In Milford Harbor, 1,000 bushels of oysters were planted on the tidal flats for a spawning bed, and over these oysters 300 lath crates, filled with shells, were set out. The crates were placed in various formations, so as to determine their value as seed collectors and the effect of their position and arrangement on the uniformity or intensity of the set. On August 15, a light set occurred in the harbor, but counting the oyster spat in the crates was not done until September 15, when they were large enough to be seen easily. The examination of the crates showed the following results:

1. An average of 2,000 spat per bushel was collected.
2. An average of 9 spat per shell was caught, giving the crop commercial value.
3. A light set, averaging 4 spat per shell, was obtained in the very center of the crates, and a heavier set, averaging 15 spat per shell, was found in the corners.
4. The maximum number of spat attached on a single shell was 35.
5. Shells in the top and bottom layers of the crate caught a heavier set than those in the middle layer.

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[3] Suggested by Capt. Charles E. Wheeler, manager, Connecticut Oyster Farms Co., Milford, Conn.

6. Crates closely packed together collected a heavier set than those set out alone.

Wareham River.—During the summer of 1926, observations and experiments with spat collectors were conducted in Wareham River, Mass., by Dr. P. S. Galtsoff and R. W. Crosley. In this region, the oystermen scatter oyster or scallop shells over the bars exposed at low water, and seed oysters usually are gathered and sold in October or November. The salinity of the water in this region varies from 14 to 29 parts per thousand; the mean tidal range is about 4 feet; the tidal currents, measured in September, 1921, are not very strong on the flats, seldom exceeding one-third foot per second.

On July 19, 50 crates were planted in various sections of the river. During the six weeks that the crates were in the water, eight of them were broken and carried out by the tide. All the losses occurred in the crates planted on the west side of the river, those on the east side sustaining the test successfully. In September the crates were examined and the seed oysters on the shells were counted.

The results of the count show (1) that setting occurs above low-water mark and was found to be heaviest about 1½ feet above the bar on which shells are planted by the local oystermen; (2) that the number of spat caught, per bushel of shell, varied from 1,900 to 45,000, according to the location of the crate.

Wellfleet Harbor.—Similar experiments with spat collectors were carried out in Wellfleet Harbor during the summer by Dr. Henry Federighi, temporary investigator of the bureau. It has been found that the type of crate used in the experiment was not suitable for regions where strong tidal currents occur. Of the 97 crates set out in various sections of the harbor, only 43 remained, the rest being washed away. Though there was no setting of commercial importance in the harbor, the crates placed in Herring River caught a fairly good set, varying from 1,200 to 2,900 spat per bushel.

The investigation in Wellfleet Harbor shows that the areas best suited for spat collection are Herring River, Duck Creek, Egg Island, and Blackfish Creek. The best setting takes place at a point midway between high and low water marks.

Briefly summarized, the results of the experiments and scientific studies show:

1. The conditions necessary for successful spawning and setting are to be found primarily in the harbors, bays, and river mouths.

2. Such inshore areas, if not grossly polluted, can be rehabilitated as prolific oyster-producing regions by the establishment of spawning beds.

3. As shown by laboratory experiments on spawning under adverse seasonal conditions, the oysters on these beds can be induced to spawn. This was successfully accomplished in Milford Harbor.

4. The oyster larvæ will remain and set in the vicinity of the spawning beds in spite of strong tidal currents and river discharge.

5. Triangular crates filled with shells can be placed on the tidal flats in the harbor and will collect from 2,000 to 50,000 oyster spat per bushel.

6. By means of the crates, from eight to ten times as many seed oysters can be collected on a given area as by ordinary methods of shell planting.

7. The crates can be planted on barren mud flats, and sand bottoms or directly over the spawning beds, thereby obtaining the maximum use of the limited inshore areas.

8. This type of crate can be used successfully, except in a few regions where the tidal currents are unusually strong.

OYSTER DRILL

At the request of the oyster companies operating in Chesapeake Bay, near Norfolk, Va., the bureau, since last October, has undertaken a systematic investigation of the oyster drill, with the view to discovering a method of checking the destruction of oyster beds by this pest. Dr. Henry Federighi, who is conducting this investigation, has established a laboratory (through the courtesy of the United States Public Health Service) at Craney Island, Norfolk, Va. The program of observations consists of a study of distribution, migration, propagation, and behavior of the organism. Field observations made during November and December show that at low winter temperature the drill becomes inactive. Further discussion of these studies may be found on page 658.

CLAMS OF THE PACIFIC COAST

During 1926 investigation of the clams of the Pacific coast was continued by H. C. McMillin, scientific assistant, and Prof. F. W. Weymouth, of Stanford University. Field work was carried on by Mr. McMillin from April to September. An examination of the commercial catch on the Washington beds indicated serious depletion. The beds at Massett, British Columbia, were examined and shells collected for growth study. Spawning was observed at Cordova, and the set of young (1-year-old) on various portions of the bed was determined. Swickshak Beach showed the heaviest set of any Alaskan bed. Data were collected here to determine the correct status of the form now described as *Siliqua patula* var. *alta.*

Shells were collected at new places, and material is now available for a study of growth on all important beds. A number of shells from animals of known sexes were measured, and norms of growth were constructed for each sex. Although the sexes are separate in this species, there is no significant difference between them when mature, and determinations made without reference to sex give valid results.

Razor clams move freely through the sand in a vertical direction. Observations indicate that they are closely confined to a limited area and do not migrate. Only part of them feed at one time and are in position to be taken by the diggers; the others remain inactive at some distance below the surface. The commercial digger covers the ground on successive days without apparent reduction in catch. It is difficult, therefore, to determine the abundance on any one bed. Depletion may show in the commercial catch, but a definite measure of the actual state of the resource can not be made. By marking the beach with permanent stakes, the same area can be dug each day and the catch recorded. A limited numbere of such observations indicate that beds that have been subject to heavy commercial digging will show a rapidly decreasing daily production.

The razor-clam beds of Washington are in urgent need of protection. Tourist and commercial digging have so reduced the popula-

tion of the beds that only very young clams are commonly found. A short report of these conditions with recommendation for a size-limit regulation was submitted to the supervisor of fisheries of the State of Washington.

After examining canned samples of *Cardium corbis* and consulting with interested operators, a suitable method of processing this clam has been worked out. By using this species it is possible for the canneries to operate during the winter, when razor clams can not be obtained, and during the late summer, when there is a closed season for razor clams. The supply of this species is quite extensive and the product is of satisfactory quality.

SCALLOPS

The scallop investigations in North Carolina, begun in July, 1925, by J. S. Gutsell, have been continued through 1926. Special emphasis has been laid on spawning, growth, and longevity.

Spawning has been found to begin in the spring (if, indeed, it ceases at all through any season) and to continue to the end of the year. However, there is accumulating evidence, chiefly from the collections of small scallops, that the principal spawning occurs over a shorter period, beginning in early fall or late summer and extending through the fall, perhaps into the winter. New methods of collecting small scallops and of examining the collected material have given greatly improved results for this as for other aspects of the work, so that good evidence throughout 1927 is anticipated.

Growth data that show remarkable homogeneity of size grouping indicate an increase in length from 1½ inches in May to about 3 inches in the next fall or winter. Commercial destruction of scallops at Pivers Island and other known sources of supply in February, 1926, prevented extension of knowledge of later growth and normal longevity. These problems we hope to solve during the coming year.

On advice of the investigator, some modification of the scallop season by the State authorities aready has been made. It is hoped that when the present studies are completed, or sufficiently advanced, detailed recommendations of practical value may be made available to the State board, which has taken an active interest and to which thanks are due for cooperation.

FRESH-WATER MUSSELS

Undoubtedly the outstanding work carried out in 1926 in connection with fresh-water mussels, of value to the pearl-button industry for their shells, was performed at the Fairport (Iowa) fisheries biological laboratory by Dr. Max M. Ellis, of the University of Missouri, a special investigator of the bureau. After working on the problem several summers Doctor Ellis succeeded in developing a nutrient solution that serves as a medium for the development of mussels from the glochidial to the adult stage. This elimination of the parasitic stage in the life history of the mussels promises to simplify greatly the propagation work that the bureau is conducting with a few of the more important commercial species. Doctor Ellis plans to develop the use of this solution during the coming summer.

T. K. Chamberlain, director of the Fairport station, went to Arkansas to represent the bureau in devising more satisfactory

regulations for the mussel fishery than the one in force. In company with representatives of the Arkansas fish commission, Mr. Chamberlain spent over three months in examining the state of the mussel fishery in Arkansas waters. A large number of shellers and shell buyers were interviewed, and tentative recommendations were drawn embodying alternate open and closed sections of the rivers, which were given publicity in the State press. Criticisms and suggestions were invited, which resulted in some minor changes, but the revised recommendations were acted upon favorably by the commission in November and are to go into effect on February 1, 1927.

The new series of sections alternately opened and closed differs from the old, mainly in that the average length per section is a little under 15 miles, as opposed to the 70 miles provided for in the program that failed. It will be possible now for all shellers who live along the river fronts to be within a convenient distance of some open territory at all times.

After completing the work in Arkansas, Mr. Chamberlain began a new series of mussel surveys in certain waters of the upper Mississippi. In these surveys it is planned to develop new methods, based upon those used by Doctor Weymouth in his studies of the salt-water clams of the Pacific coast.

A survey of the mussel beds of certain rivers in Virginia was made by H. O. Hesen, superintendent of fish culture at the Fairport station, to determine the effect of former plantings of commercial mussels taken from the Mississippi River. Mr. Hesen made fresh planting of several thousand young mussels, reared by the trough-culture method, which was employed on a small scale at Fairport during the summer. There was no indication that commercial mussels had become established in Virginia waters.

In connection with the studies of the life history of the more valuable fresh-water mussels, a particular study of the habits of the two species of gar found in the Mississippi in the vicinity of Fairport was made during the past summer by Doctor Ellis. One or both species of gar are the hosts for the glochidia of the most valuable of all the fresh-water mussels—the yellow sand shell (*Lampsilis anodontoides*).

TERRAPIN CULTURE

The experimental work in breeding diamond-back terrapins at the Beaufort (N. C.) fisheries biological station continued to give interesting results. Experiments in hybridizing Carolina and Texas terrapins were started in 1915. It was hoped that in cross-breeding the two species, a fast-growing animal with a flavor scarcely inferior to that of the Carolina terrapin might be produced.

A cooperative arrangement for hatching terrapins, entered into with the fisheries commission board of North Carolina in 1925, has been extended. An additional concrete pound, 125 feet in length and 64 feet in width, was constructed to hold 1,235 breeding terrapins, which the State has supplied. It is purposed to hatch a large number of terrapins and to hold these young animals at the station until they have attained a considerable size and have passed through the most critical stages of life, when they are to be liberated for restocking the sadly depleted waters. Several hundred young animals were liberated in the vicinity of Beaufort from 1914 to 1924, and as

several of these animals have been recaptured the indications are that a fair percentage survives to reach maturity. In 1926 the surplus young, not needed for experimental purposes, were turned over to the State fisheries commissioner, and 1,881 individuals were liberated by him in suitable places in the sounds of North Carolina.

The total number of young terrapins secured in the fall of 1926 was 4,370, which is an increase of 1,402 over the hatch of 1925. All of these young, except 735, are the offspring of the bureau's experimental stock. The terrapins belonging to the State, in part, were received too late for the last breeding season, and the rest had not been in confinement long enough to have become acclimated. A much larger number of young, therefore, is expected in 1927. The diamond-back terrapin work is under the supervision of Dr. Samuel F. Hildebrand and in immediate charge of Charles Hatsel.

FISHERIES BIOLOGICAL LABORATORIES

Thirty investigators and others took advantage of the opportunities offered at the United States Fisheries Biological Laboratory at Woods Hole during the summer season of 1926.

Dr. J. O. Snyder, head of the zoology department of Stanford University, acted as director. The station was used to an unusual degree by permanent and temporary employees of the bureau, and research facilities were afforded to investigators from other Government departments, also, and to research workers from widely separated universities and colleges. Doctor Snyder expressed himself as in full sympathy with the change in policy with regard to privileges of this laboratory, inaugurated last year, whereby, in case of overcrowding, investigators are selected on the basis of the scientific program contemplated, accommodations being afforded only to those who are working on problems of special interest to the bureau and who have shown capacity for energetic and productive research. He expressed astonishment at the richness of the opportunities the station offers for the study of marine animals, suggesting that the failure to receive more numerous applications for these facilities must be due to misunderstanding or lack of information on the part of the younger investigators of the country.

The laboratories, library, and apparatus were in good condition, and apparently the needs of the various investigators were well taken care of. Boats and collecting apparatus were available at all times. The aquarium was in fine condition, and live material was supplied promptly upon request of investigators. The particularly efficient and willing service of Robert A. Goffin, the station collector, and Capt. Robert Veeder, of the steamer *Phalarope*, deserve mention. Raymond G. Hoffses, superintendent of the station, and Miss Jessie E. Drayton, who acted as secretary, assumed charge of the many details, which contributed to the general efficiency of the laboratory. Miss Regina M. Ford was active in the library. She brought together the peridodicals and scattered books, catalogued the separates that had accumulated, and arranged the duplicates on newly installed shelves. Alvin S. Eichorn placed the storeroom in order and made the customary inventory of material and apparatus.

In 1926, as during the previous year, the laboratory was again the headquarters of the oyster investigations conducted by Dr. P. S.

Galtsoff and his assistants. Doctor Galtsoff and H. R. Seiwell worked largely on the physiology of feeding and reproduction in oysters, and field observations on various problems in oyster culture were conducted by Dr. Henry Federighi and J. H. Weatherby.

The laboratory was made the center of the mackerel investigations, also, under the direction of O. E. Sette. W. C. Schroeder conducted his investigations on the life history of the cod and haddock and completed his manuscript on the fishes of Chesapeake Bay, which is to be published in collaboration with Dr. S. F. Hildebrand.

Dr. C. J. Fish and Marie P. Fish continued their investigations of the eggs and larvæ of the cod and other larval fishes occurring in the Woods Hole region.

Dr. F. G. Hall, of Duke University, assisted by Dr. Samuel Lepkovsky and Dr. Irving E. Gray, continued their excellent researches in the metabolism of fish, with reference to various degrees of salinity of water.

In addition to the staff of the bureau, researches were conducted by numerous private investigators. Dr. N. A. Cobb, nematologist, of the United States Department of Agriculture, continued his studies, with the aid of four assistants, on the nematode fauna of Woods Hole region. Dr. Edwin Linton and Dr. G. A. MacCallum carried on their studies of fish parasites, as they have done for many years past. Paul S. Conger, of the Carnegie Institution of Washington, working under the direction of Dr. Albert Mann, continued the study of the diatom flora, with particular attention to the bottom forms. Dr. C. B. Wilson carried on important studies in both parasitic and free-living copepods, and among other things completed the examination of a large number of collections made by the *Albatross* and other research vessels of the bureau.

Earle B. Perkins occupied the Harvard table while engaged on color changes in Crustacea. Dr. H. B. Stough, Dr. C. J. Connolly, Dr. W. E. Bullington, E. F. B. Fries, Dr. F. M. Baldwin, and E. G. Agersborg were also engaged on private researches. Dr. N. Borodin, of the Brooklyn Museum, visited the station in connection with his museum work.

The fisheries biological laboratory at Fairport, Iowa, has already been mentioned in connection with the work on the commercial freshwater mussels, and also in connection with the studies in aquiculture made by Russell F. Lord under the direction of Dr. H. S. Davis. These are the major activities, but the station, under the direction of T. K. Chamberlain, has been the center of other important activities.

A. H. Wiebe, a special investigator for the bureau, made a biological survey of the upper Mississippi during the summer, using Fairport as headquarters. This investigation, while under the direction of the bureau, was financed largely by the States of Minnesota and Wisconsin and the municipalities of St. Paul and Minneapolis. The plankton collections made will be studied at the Fairport station by Mr. Wiebe.

An investigation begun by Dr. R. E. Coker some years ago, when he was director at the Fairport station, was taken up again during the past summer by Doctor Coker, at the bureau's request. This was a study of the effects of the dam across the Mississippi at Keokuk, Iowa, upon the fish population above and below the dam.

Work of special interest to the bureau was performed by a private investigator, George W. Hunter, of the University of Illinois, who occupied a table at the Fairport station throughout the summer. Mr. Hunter worked on fish parasites found in the vicinity of Fairport. In the course of his investigations he discovered a new species of tapeworm and a new species of nematode.

In connection with the aquicultural studies of Doctor Davis and Mr. Lord, the station was able to conduct considerable fish-cultural work on warm-water game fish. The fish produced were turned over to the division of fish culture.

The activities of the fisheries biological laboratory at Beaufort, N. C., were extended during the year. Dr. S. F. Hildebrand, the director, assisted by Charles Hatsel and Irving L. Towers, carried on experiments in diamond-back terrapin culture, the study of the life histories of several species of fish, and observations and experiments relative to the use of fish for controlling mosquito breeding.

A collection of 38 species of fish was made in the vicinity of Greenwood, Miss., in connection with investigations pertaining to the use of fish for mosquito control, carried on at that place during the summer of 1925. The fish have been identified, and a few undescribed forms appear to be included. The stomachs of many of the specimens have been examined for the food contents and a report on the collection is being prepared.

Investigations relative to the use of fish for controlling mosquito breeding, carried on for a number of years, were curtailed, owing to the urgency of other duties. Only limited observations and experiments, concerned principally with mosquito breeding in brackish water, were made in the vicinity of Beaufort.

Elmer Higgins, chief of the division of scientific inquiry, assisted by R. O. Smith and others, continued his investigations on the mullet fishery and of the Pamlico Sound fisheries from August to October. J. S. Gutsell continued his studies on the life history of the bay scallop.

Toward the end of the year, H. R. Seiwell and R. W. Crosley were detailed to Beaufort to study local oyster problems. The Navy Department sent two investigators, Dr. A. W. Bray and Dr. J. Paul Visscher, to the station in the summer to continue their previous studies on the prevention of the fouling of ships' bottoms. In addition, six independent investigators availed themselves of the privileges of the laboratory.

Prof. H. V. Wilson, of the University of North Carolina, made studies on the behavior of sponge cells; Dr. Bartgis McGlone, of the University of Pennsylvania, continued his studies of the previous summer on the effects of hydrogen-ion concentration on the fertilization of the eggs of sea urchins; Dr. Hoyt S. Hopkins, of New York University, studied respiration in the tissues of mollusks; Dr. Elinor H. Behre, of the University of Louisiana, studied color adaptation in fishes, especially in certain blennies and in a foolfish; Dr. Libbie H. Hyman, of the University of Chicago, worked on digestion in sponges; and H. Randolph Halsey, of Columbia University, made some studies pertaining to the fertilization of the eggs of the stone crab, *Menippe mercenaria*, and he also made studies of the effect on certain cytoplasmic bodies of the cell of centrifuging the eggs of sea urchins.

FIG. 1.—Officers and scientific staff of the Bureau of Fisheries present at the divisional conference. Reading from left to right: Herbert F. Prytherch, Dr. Henry Federighi, Mrs. Chas. J. Fish (Marie D. Fish), Dr. Chas. J. Fish, Wm. C. Schroeder, Dr. Henry B. Bigelow, Russell F. Lord, Dr. Walter Koelz, John C. Pearson, Dr. Paul S. Galtsoff, Robert O. Smith, Henry O'Malley, commissioner, Robert A. Nesbit, Irving L. Towers, Dr. Samuel F. Hildebrand, Dr. John Van Oosten, Dr. Herbert S. Davis, Dr. Wm. C. Kendall, Lewis Radcliffe, deputy commissioner, Dr. Chas. H. Gilbert, Dr. Willis H. Rich, Elmer Higgins, assistant in charge of scientific inquiry, Thos. K. Chamberlain, James S. Gutsell, Geo. A. Rounsefell, Capt. F. W. Wallace, Harlan B. Holmes, Frederick S. Norman, Milton C. James, H. Richard Seiwell, Isaac Ginsburg, Oscar E. Sette, assistant in charge of fishery industries, N. B. Scofield, Hermon O. Hesen, Abraham H. Wiebe.

Part II.—PROCEEDINGS OF THE DIVISIONAL CONFERENCE, JANUARY 4 TO 7, 1927

SYMPOSIUM ON FISHERIES AND FISHERY INVESTIGATIONS

OUR OPPORTUNITIES: OUR RESPONSIBILITIES

By Henry O'Malley, Commissioner of Fisheries

I am very glad to welcome you to this first conference of the division of scientific inquiry. We have an important work to perform, and I trust that the results of this conference will be such that we will want its annual repetition.

After you have had an opportunity to examine in some detail the work of the various investigators, I hope that you may be able to direct your efforts toward the most worthy goals, getting a perspective of the larger problems of the fisheries and realizing their gravity and importance to the Nation. You should have a self-consciousness as fishery investigators and a group consciousness as the scientific staff of the Bureau of Fisheries that will add to your respect for the dignity and significance of your daily tasks and increase your pride in your chosen calling.

You fishery investigators are favored persons. You have unprecedented opportunities, but with these opportunities there are equally great responsibilities.

The attitude of the people toward scientific investigation of the fisheries has undergone a distinct change in the last five years. While the tendency was noticeable many years before, it has been only very recently that we find dealers and fishermen, leaders of the industry, advocating fishery regulation, requesting technical advice, and calling upon us to draft appropriate legislation to protect the fisheries. While the fishery investigator formerly was looked upon as some kind of a queer, impractical person, a "bug hunter," he is now regarded by the more intelligent fishermen as an expert with a deeper insight and broader, more sympathetic understanding of the problems of the sea than many so-called "practical" men. This change of attitude on the part of the public has been reflected in the legislation of the States and Congress by increasing appropriations for research. While formerly congressional constituents demanded only the establishment of fish hatcheries, now, particularly in the marine fisheries, the demand is also for biological investigations, and funds for this purpose are being provided.

The fishery investigator of to-day stands on the threshold of a new era of scientific development. The new science of fishery biology is developing rapidly, both in this country and in Europe, and promises to become an important and honored member of the group of natural sciences. While systematic ichthyology in America is based upon the names Gill, Jordan, Gilbert, and Cope, the future development of fishery biology may well rest upon the names of many of you here present. Fishery biology is entering upon a virtually virgin field; there is no dearth of urgent and significant questions; in fact, every fishery in every section of the country offers a host of problems too numerous to mention and as yet untouched. Not only the practice of fishery conservation but the very fundamentals still await investigation.

The opportunities for fishery research offered by the Bureau of Fisheries are now superior to those ever before provided, and, needless to say, are superior to those of any private organization or institution. Few institutions have the financial resources of the Government for such work; none of them have the facilities for collecting data or the authority to make these data accessible. Few have access to library facilities equal to those found in Washington, and few can offer their investigators such freedom of movement, such breadth of field, as can the Bureau of Fisheries. The bureau investigator may pursue his subject to its ultimate and complete solution and satisfy the desire for scientific

recognition, which so often is a powerful spur to activity. The bureau's employment offers opportunity for the making of a career. Academic honors and scientific recognition are now possible, and adequate compensation and the possibility of material advancement make the bureau's positions most desirable.

But along with all these opportunities, as I have said, come heavy responsibilities. We can not deny or ignore the fact that our fisheries are declining. A half century of the bureau's activities have not been sufficient to prevent depletion in some, commercial extinction in others. I should not care to say that the bureau's work has amounted to nothing, but we must conclude that our efforts have not been sufficient to maintain the fisheries in their former state of productiveness. We must, therefore, extend and more wisely direct our efforts; we must succeed in coordinating and organizing the efforts of our staff and of biologists throughout the country upon the problems of the maintenance of the annual yield.

Our responsibility toward the fisheries is emphasized by the childlike faith of the public in the efficacy of science. Science in industry has worked wonders. Science is introduced into business, into government, into every phase of daily life; and it is natural and, indeed, proper to expect science to maintain the fisheries for all time. This confidence is almost embarrassing, and in view of the complexity of the problems of conservation, this faith may well be shaken by the unavoidable slowness with which results are produced.

But results can be produced, I am very confident, through cooperation and organization. The division of inquiry must develop a comprehensive program of fishery investigation, in which each of you must take a part as a unit in a great machine.

It is my ambition to see this scientific staff grow in numbers and in effectiveness far surpassing previous experience. I want men of ability and vision, of industry and diligence, who are prepared to put the whole of their energies and interests into the shaping of their scientific careers in the bureau. Half interest and half time can never bring the results for which the bureau aims, and the dilettante naturalist can expect no Government subsidy through this bureau.

It is through cooperation that the great responsibility of your division can be discharged effectually. The principles of fishery investigation must be carried into execution, made applicable to the daily problems of fishery administration, and, above all, must produce results for the benefit of the fisheries. The individual investigator may be concerned with a theoretical problem, but even in the prosecution of highly technical scientific work the ultimate aim of fishery conservation must be kept clearly in mind. Some must develop principles, some must work upon their application, but the aims and objects of our work must be ever before us. We are all giving to the people of this country a service as real and as important as that of any agency of production. This service to be enduring and far reaching must be carried to the ultimate consumer or it fails completely.

Your immediate problem in this conference is to devise means and develop methods of effecting real fishery conservation, and I charge you with the responsibility of perfecting a program of action that will be more effective than any hitherto developed. I have no anxiety concerning your success, and I unhesitatingly place in your hands the development of the scientific work of this bureau, confident that through your whole-hearted endeavors the fishery industry and the American people will receive benefits that will be a source of pride to the bureau and to yourselves.

Mr. Higgins. Mr. O'Malley has laid down our work for us very pointedly, indeed. We all have our own ideas of how fishery conservation should be effected. It is now our duty, as he has told us, to coordinate these ideas, and therefrom to develop a policy. Before attempting to lay down a general policy for the bureau, it is necessary for us, I believe, to examine, as a background, the field in which we must work, and I have asked Mr. Sette, in charge of the division of fishery industries, who has at his command all of the available statistics, to review for us just what can be found out about the actual state of America's fisheries.

THE STATE OF AMERICA'S FISHERIES

By O. E. Sette
Assistant in charge, Division of Fishery Industries

The first thing we must face, in considering the status of our fisheries, is that we have an invisible resource. One can cruise timber and learn the remaining stand, and one can enumerate the acres of cultivated land, but the fisheries one can not see. All we know about the fisheries is what the fishermen bring to market, and therefore, in considering this subject, we have to deal entirely with the fisherman's catch. Moreover, it is only by a consideration of the past that we can understand the present or foresee the future, so that this discussion will deal largely with the record as it appears in the published statistics of the fisheries.

Our first records of this nature were made in 1880, 46 years ago, when the first census of the fisheries was taken. Since then there have been periodical censuses of the various regions. I have charted (fig. 2) a picture of these censuses.

The squares in the horizontal row across the top represent the years for which we have statistics of the fisheries of New England. You can see that they are scattered pretty well during the period. On the line below are the Middle Atlantic States. Below that are the South Atlantic, Gulf, Pacific coast, Great

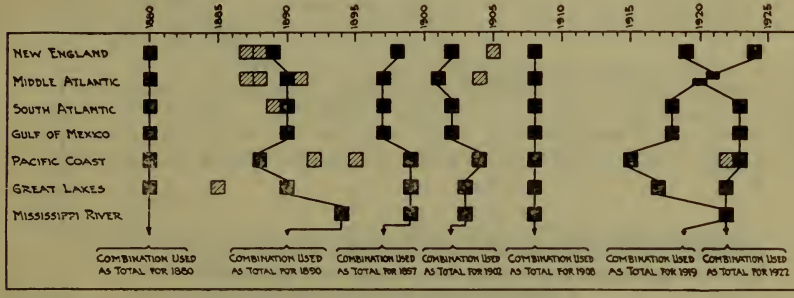

FIG. 2.—Years for which statistics are available on the fisheries of the various geographical sections of the United States, and combinations that were used in compiling totals

Lakes, and Mississippi River. You will notice immediately that there are only two years for which we have the statistics of the whole country—1880 and 1908. Even 1880 is incomplete because we haven't the Mississippi River. The remaining years are so scattered that it is impossible to compile the statistics of the country for any one year to show the total yield; but as a substitute I have compiled those that apply most nearly to the same year and include all of the regions as indicated on the chart. It is an imperfect compilation, but it is the best available.[4]

Referring to Figure 3, in 1880 the total yield of our fisheries, exclusive of Alaska, was less than 1,600,000,000 pounds. This has risen, with some fluctuation, to a total at the present time of nearly 2,200,000,000 pounds. The line of dashes represents the yield in the Atlantic coast section including the Gulf, which has fluctuated widely but shows an upward trend, running from about 1,500,000,000 to over 1,600,000,000 pounds in recent years. You will also notice that the Atlantic coast yields the greatest poundage of fish. The Pacific coast has grown from about 50,000,000 to about 400,000,000, an increase of approximately 800 per cent during the period covered. The Great Lakes and Mississippi River together have yielded about 150,000,000 pounds annually from 1890 to the present time.

[4] In order to provide comparable statistics in the various years, all salt fish appearing in the reports has been converted to the equivalent amount of fresh fish. The statistics on oysters, clams, and scallops are in terms of meats, exclusive of shells. Statistics on seed oysters, shells, hides, oils, whale products, king crab (*Mimulus*), and frogs have been omitted.

Confining our attention now to the Atlantic coast, let us examine the components of the present-day catch of fish.

The largest single item in the catch is menhaden. Of the total catch (1,638,000,000 pounds), 770,000,000 pounds were of this species. Referring to Figure 4, we see that the poundage of menhaden was greater than that of all other fish combined and more than twice that of all shellfish. This great predominance of menhaden is not often appreciated, probably because this fish has not been used for food and thus is considered a "poor relation" among the fishes. But, despite our low esteem of this fish, it must certainly be a tremendous factor in the ecology of the sea. In fact, the relative amount in the catch probably underrates it as compared with other fish. For menhaden the fishermen receive only one-half cent a pound. For other fish the average price is 4 cents. If the fishermen were to get 4 cents a pound for menhaden, it would loom still larger in the catch.

Cod and haddock rank next in quantity to menhaden, each yielding over 90,000,000 pounds annually. Herring is fourth in quantity, with over 60,000,000

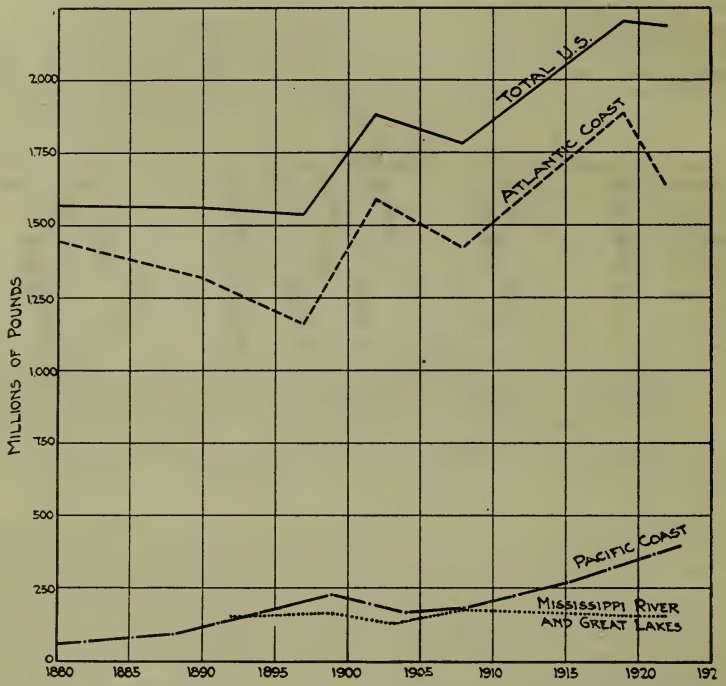

FIG. 3.—Yield of the fisheries of the various waters of the United States

pounds. The catches of mullet, alewives, squeteagues, and flounders are nearly equal—close to 40,000,000 pounds annually. Others follow, in order of quantity, as shown in Figure 5. It is interesting to note that two of our most esteemed fishes—halibut and bluefish—are near the bottom.

In order to get a group picture of the changes occurring in our fisheries I have compiled the statistics of several branches of the fisheries, grouping separately the fresh-water, anadromous, catadromous, shore, demersal, and pelagic fishes. Some explanation of these groupings may be in order. The fresh-water fishes include all those that nominally spend their lifetime in the rivers and are largely caught there by commercial fishermen, though they may be taken occasionally in brackish water. Some of the important species in this group are suckers, carp, catfish, sunfish, yellow perch, black bass, pike, and buffalofish. The statistics of these may not be complete, for the canvasses include the coastal streams only so far as the commercial fishery is relatively important. Among the anadromous fishes are the shads, alewives, striped bass, white perch, smelt, salmon, and sturgeon. The common eel comprises the catadromous

group. Among the shore fishes I have included all species usually taken by inshore gear, such as pound nets, traps, weirs, haul seines, hook and line, and gill nets operated in inshore waters. Thus, the sea herring, which has some claims for inclusion among the pelagic fishes, was put into the shore group because it is taken mostly in weirs; and pollock, which is regularly taken among the demersal fishes, has also been included with the shore group because by far the largest part of the catch comes from inshore waters. Other species of importance in this group are squeteague, mullet, croaker, spot, butterfish, drum, king whiting, scup, and whiting (silver hake). Among the demersal

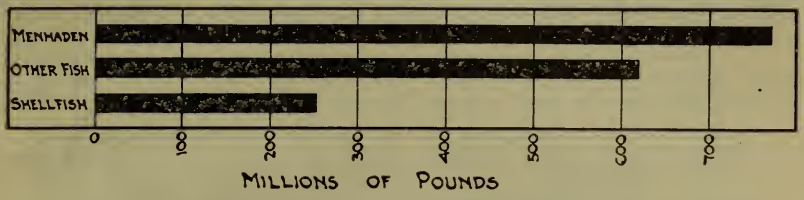

FIG. 4.—Relative size of the menhaden catch

fishes I have included cod, haddock, hake, halibut, flounders, snappers, groupers, etc. Among the pelagic fishes I have included all those taken primarily in offshore surface waters by such gears as purse seines, drift gill nets, and harpoons. These are menhaden, mackerel, swordfish, etc.

By consulting Figure 6 we may follow the changes in yield in these various groups. As might be expected, the group of pelagic fishes shows wide fluctuations from 340,000,000 to 930,000,000 pounds in various years. It also has had a decided upward trend since 1908. The demersal fishes as a group have had a fairly constant yield since 1890. The shore fishes reached a peak yield

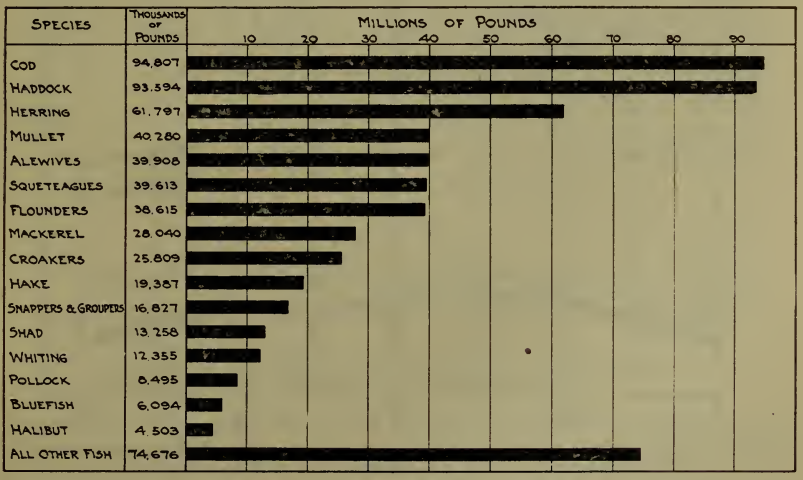

FIG. 5.—Relative quantities of various species of fish (excepting menhaden) taken annually on the Atlantic coast

in 1902 but have declined since. The anadromous fishes reached their peak yield prior to 1897, but have since then declined more severely than the shore fishes. There seems to be an orderly coherence in these trends when considered in terms of the accessibility to the fishermen of the various groups. The least accessible are the offshore surface fishes, which roam over relatively large areas and may be found only by much cruising in search of the wandering schools, and are caught with difficulty when found. These show only a tremendous fluctuation in yield and the trend, if any may be distinguished, seems upward. Next are the demersal fishes, found over extensive offshore areas

but caught with greater certainty. These show a relatively constant yield. The shore fisheries are confined to a more limited area conducive to a more thorough exploitation. The fish in this group passed the climax of their yield in 1902 and since then have been taken in smaller quantities. The anadromous fishes are still more strictly confined to limited areas during their spawning runs when they are subject to intensive fishing, and their yield passed its climax some 10 years earlier than that of the shore fishes and has declined more severely.

We can not venture too far with general conclusions of this sort, however. Each of the above-mentioned groups is a complex of species that must be examined separately. For this purpose I have prepared a number of charts

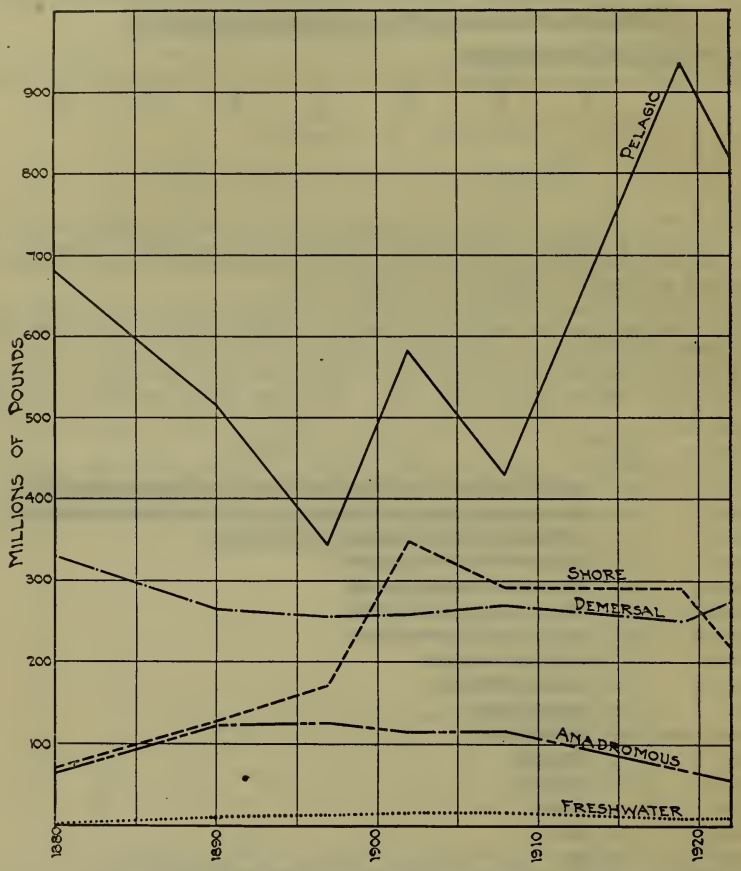

FIG. 6.—Yields of various groups of fishes on the Atlantic seaboard, 1880 to 1922

on a logarithmic scale to show the relative changes in yield of the more important species. The logarithmic scale reduces the curves to a convenient form for comparing rates of change, and in interpreting them the relative steepness of the slopes of the curves is the significant feature. Thus, if in a given period the yield of a species increases from 5,000,000 to 10,000,000 pounds, a 100 per cent increase, and another species increases from 12,000,000 to 24,000,000, also a 100 per cent increase, the slopes of the two curves will be the same. In other words, logarithmic plotting expresses the percentage increase or decrease.

Among the pelagic fishes (fig. 7), menhaden and mackerel fluctuate so much that it is difficult to say whether the trend is upward or downward. It seems slightly upward in the case of menhaden and slightly downward in the case of

mackerel. Swordfish present a slight upward trend and Spanish mackerel (including cero) show a distinct downward trend.

Among the demersal fishes (fig. 8), cod and halibut show a general downward trend (more severe in the case of halibut), haddock show a general upward trend, as do also snappers and groupers, flounders show a most decided upward trend, and hake showed a slight upward trend until 1897 and thereafter a moderate downward trend.

All of the important anadromous fishes (fig. 9) show trends distinctly upward in the early years and downward in the later years. Shad and sturgeon reached peak productions earliest (1897) and declined most rapidly thereafter. Smelt reached peak production as early but declined less consistently. Striped bass and alewives, though showing a declining yield, have not fallen as rapidly and consistently as the others.

Among the shore fishes (fig. 10), four species—whiting, butterfish, spots, and croakers—show very marked upward trends. Scup and kingfish seem to

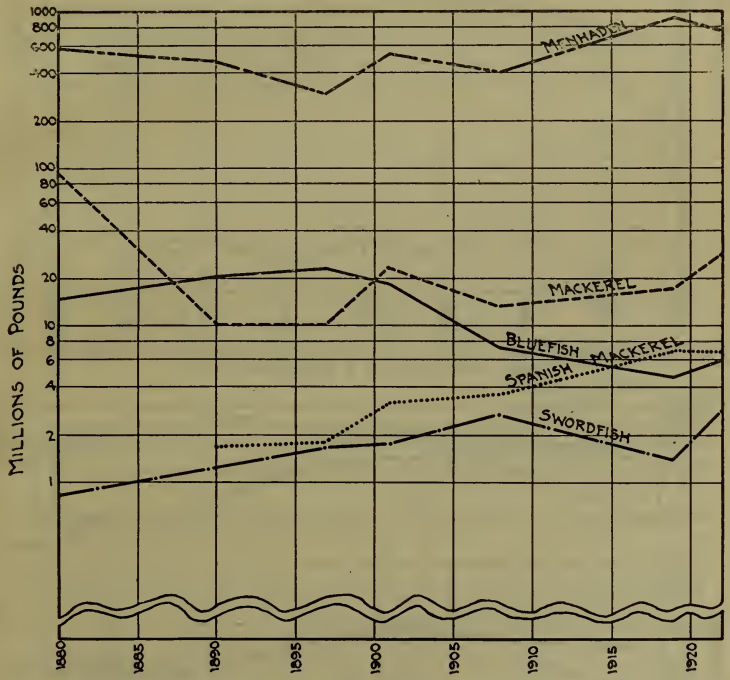

FIG. 7.—Changes in the yield of several important pelagic species on the Atlantic seaboard (on a logarithmic scale)

have maintained their yield at a fairly constant level. Squeteague and mullet grew in yield until 1902, and have since maintained it at nearly the same level. Herring and pollock show a rise and fall of considerable magnitude.

If we group the species that have declined most seriously in recent years (bluefish, cod, halibut, shad, and sturgeon), we find that they are all fish that were highly prized during early years and have been subject to intensive fishing for the longest time. Also, if we group those showing greatly increased yield in recent years (flounders, Spanish mackerel, haddock, snappers, groupers, croakers, spots, butterfish, and whiting), we find that they are species less in demand in former years but that have been exploited more lately. It would seem that in general the yield on the Atlantic coast has been held at a high level by replacing the staple fish of former days with new species, which were not greatly in demand in the earlier years. In some cases this may have been due to economic factors. The cod, for instance, among those that have declined, apparently has been affected by the decreased salt-fish trade; but in most cases the declining yields seem to be the result of depletion, and the total yield of

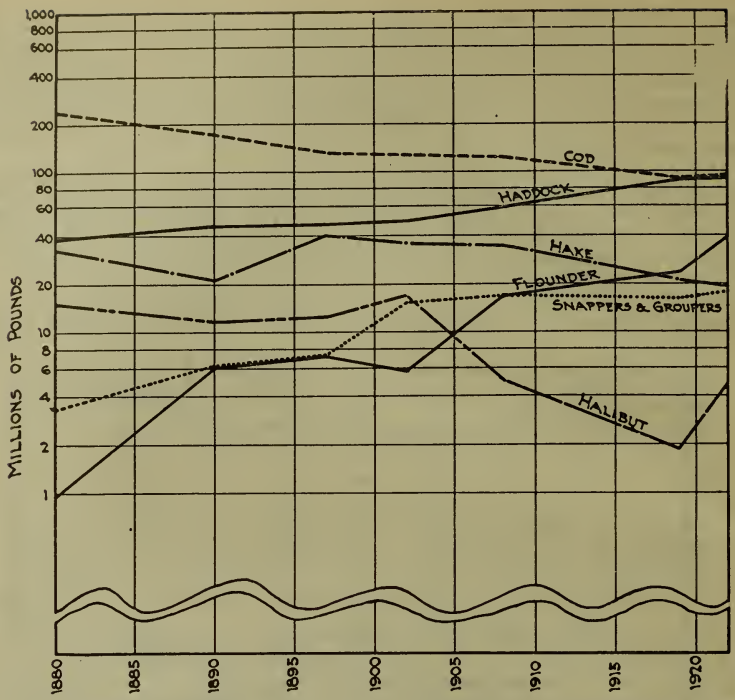

FIG. 8.—Changes in the yields of several important demersal fishes on the Atlantic seaboard (on a logarithmic scale)

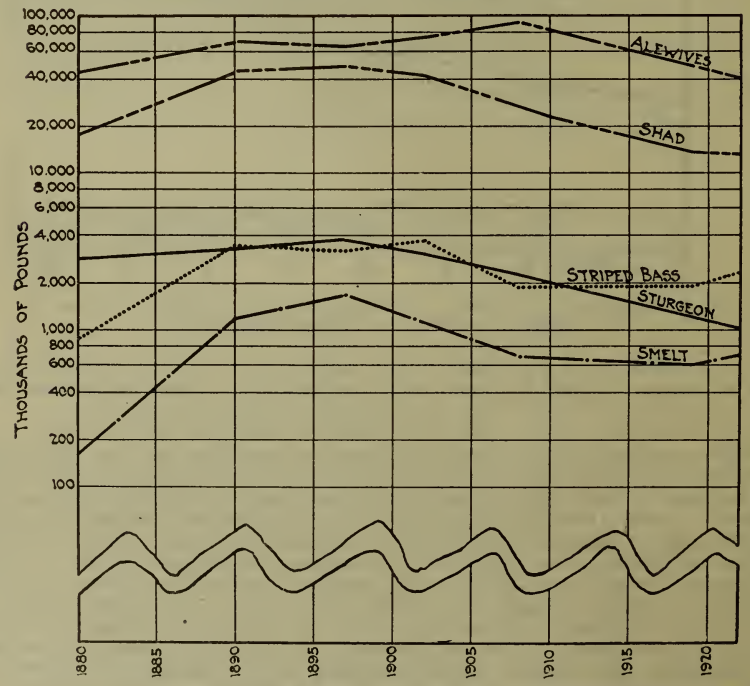

FIG. 9.—Changes in yields of several important anadromous fishes on the Atlantic seaboard (on a logarithmic scale)

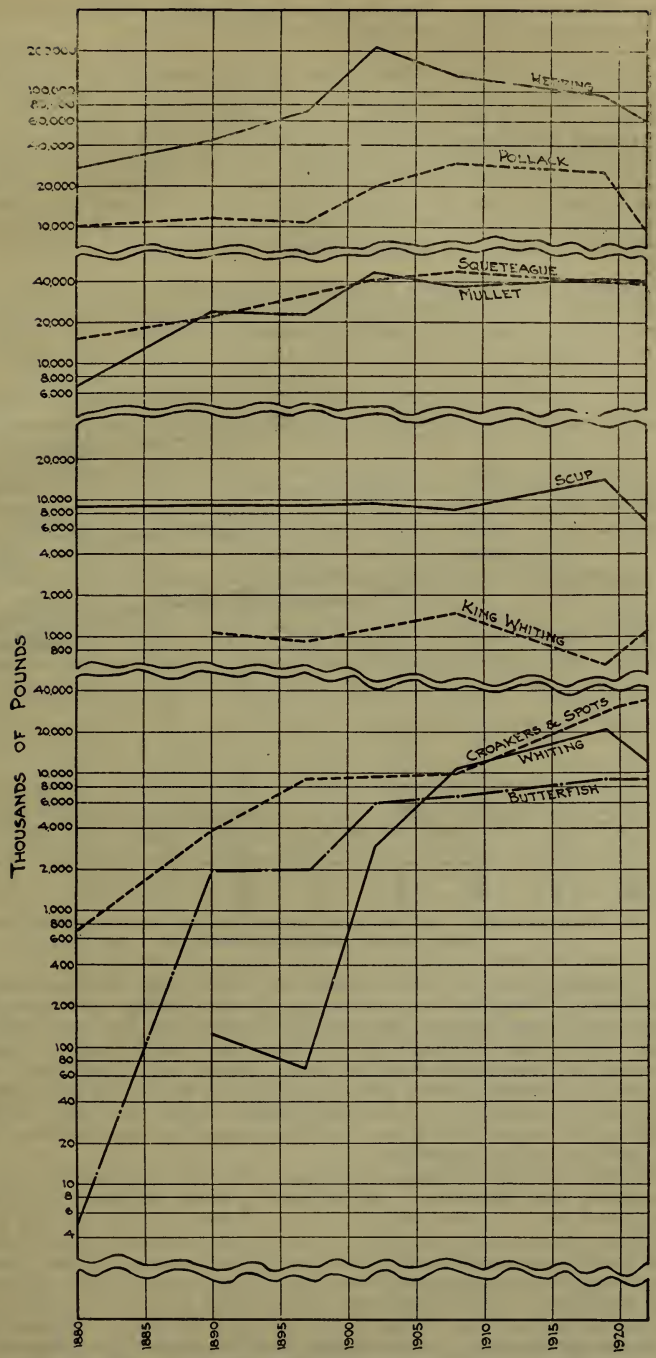

FIG. 10.—Changes in yield of important shore fishes on the Atlantic seaboard (on a logarithmic scale)

our fishes is being maintained only by drawing on new resources. If so, we have a disquieting picture. Our substitute species may decline when they are subjected to intensive fishing as long as the others have been.

Among the shellfishes we find similar tendencies. Referring to Figure 11, we see that oysters, crabs, and clams have declined moderately; lobsters have declined more; while shrimp is the only shellfish that has an upward trend in yield. Oysters, clams, and lobsters were fished more extensively in the earlier years and they began their decline first. The most intensive fishery for crabs began when the dredge was introduced in the early nineteen hundreds. This kept the yield up for over a decade, when it too declined. Similarly, the otter trawl, introduced into the shrimp fishery after 1908, has been responsible for greater yields, which are still continuing to increase. If it were not for the greater yield of shrimp, the shellfish fisheries would show a distinct decline since 1908.

Similar charts could be shown to demonstrate trends of the same nature in fisheries of other regions of the United States, but it will not be necessary to trouble you with the details. Suffice it to say, that while the Pacific Coast States show a remarkable upward trend in the total yield of their fisheries, the

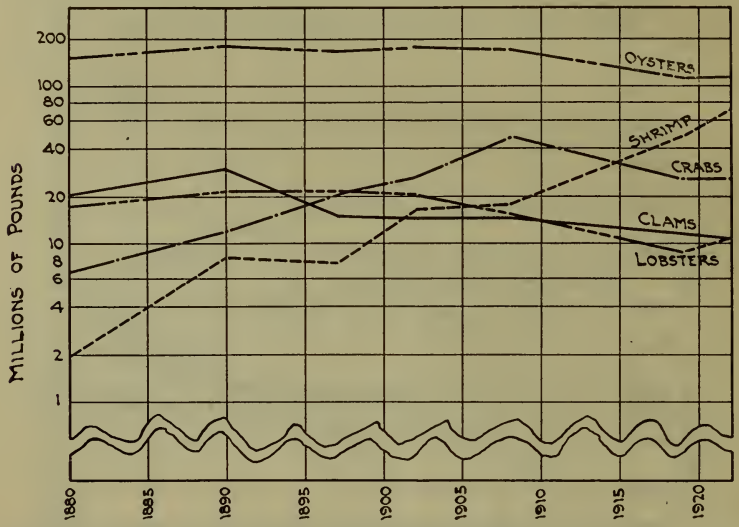

FIG. 11.—Changes in the yield of the important shellfish on the Atlantic seaboard (on a logarithmic scale)

salmon and halibut, which have been fished the longest time, are barely maintaining their yields. The great increase is due to the sardine and tuna fisheries of comparatively recent origin. As for the Great Lakes, Doctor Koelz, in his report, has shown that although they have a relatively constant yield, this constant yield has been maintained by the increased use of so-called "rough" fish, which to a marked extent, have replaced the fishes that formerly were considered more desirable. In the Mississippi River, we have a rise and decline during the period, and if we omit the mussel fisheries we find that the yield has declined quite markedly.

In conclusion, I wish to emphasize again that yield statistics have been used throughout this discussion. According to these statistics, our yield has been maintained and in some regions increased, but not by general increases in all species. On the contrary, many of the highly prized species have failed to provide yields comparable with those of former years. Their places have been taken by species that formerly were not sought after. While this does not mean that depletion is proven in every case where decreased yields occurred, it does point out the fact that there is need for critical studies of many species. If the yield of our fisheries is to be maintained, we must determine first what fluctuations in abundance are occurring, then find out whether they are due to natural or other causes, and finally determine what steps should be taken to

conserve those species that are being depleted by agencies under the control of man. Until this is accomplished, we will not know the exact status of our fisheries

Mr. Higgins. I think that in addition it would be desirable for us to consider, from the angle of the fishing industry, some of the problems that need early solution. I believe that there is no one more closely in touch with the industry itself than Captain Wallace, editor of the Fishing Gazette, and he has consented to discuss with us some of these problems.

OUTSTANDING PROBLEMS OF THE FISHING INDUSTRY

By Capt. F. W. Wallace
Editor, Fishing Gazette, New York

The outstanding problem confronting the fishing industry to-day is that of providing a profitable market for all that our fishermen catch. The prosperity of the industry rests upon two things—(1) larger markets and (2) utilization of inedible fish and fish waste.

Dealing with the subject of larger markets, it undoubtedly is a fact that the people of this country are not consuming as much fish as are the citizens of other nations. This condition is all the more remarkable when it is considered that the United States possesses and has ready access to more prolific and more varied fishery resources than has any other nation.

In presenting my ideas to a body of men well versed in the history of the fishing industry, it will not be necessary for me to go into the details of present-day conditions. We all know that the demand has been for certain species, many of which are becoming scarce. The task before the industry is to expand the market for those varieties of fish than can be produced in abundance and at a reasonable price. On the Atlantic coast the haddock, cod, hake, pollock, cusk, skate, flounder, herring, whiting, and mackerel fall into this class, and there are many others—good, edible fish, but not appreciated as such—for which a market should be found.

Any expansion in the domestic market is not going to be made entirely by insisting that the public use more fish in their diet, but is more likely to be brought about by a radical change in current methods of marketing. Acquainting the public with the value and variety of fish as a food is necessary. Advertising campaigns are being conducted by various branches of the fishing industry, and these are worth while and comparable with the high quality of salesmanship employed by other food-producing industries. However, the main plank of the expansion platform is to give the public what it wants, and it doesn't want bones.

To my mind, fish bones, more than anything else, have deterred people from eating fish. Prehistoric man may have delighted in sharpening his teeth upon the bones of his rude provender, and some of our near ancestors were not averse to worrying a herring, but in this day of mush foods, seedless and stoneless fruits, filet mignons, and similar "easy-to-eat" fare, anything that has to be dissected upon the plate is regarded with disfavor. I have questioned children, asking them if they like fish. In most cases the answer was "Yes, but I don't like the bones." This fact has a great deal of influence with the mothers. They are always fearful that the children will get fish bones in their throats. As a result, fish very often is passed by in favor of other foods. The same is true of older people. They consider fish messy when they are compelled to separate meat from bones. In this hasty age, extra labor in cooking and eating is avoided.

Personally, I am very fond of fish. I invariably have fish for lunch at the lunch club maintained by the dealers in Fulton Market. At this club, naturally, we have the best of fish at all times, but I have noticed that my preference, and that of most of the members, is for filleted fish. Canned salmon, fresh salmon steaks, cod steaks, halibut, and swordfish owe much of their popularity to the fact that they are devoid of small bones; while the haddock fillet, the greatest advance made in the fish business in recent years, is now being marketed so extensively largely because it is boneless.

The fresh-fish fillet, as now produced in Boston, New York, and elsewhere (a piece of solid fish meat, skinless, boneless, wrapped in parchment paper, and shipped in handy packages) has proved that the American public will eat more fish if it gets it in a fashion that will insure less trouble in handling, preparing, and eating it; and when one notes the favorable reception accorded fish marketed in this manner, it is not impracticable to state that all fish, whenever possible, should be filleted at producing points and marketed as a piece of solid meat, wrapped in parchment or packed in such fashion that it can be handled with a minimum of trouble by the retail dealer, the chef, and the housewife. My observations over many years, in connection with various branches of the fishing industry and fish trade, have convinced me that unless we market most of our fish in this manner we shall never succeed in creating the larger market we hope for.

There is yet another obstacle to overcome in enlarging the market. This is the matter of quality—of freshness. The fresh fish, as received by the housewife or the restaurant in most localities, is a burlesque of the real thing. Even in the best hotels, we, who know fish, receive portions that are decidedly stale and unappetizing. A vast amount of stale fish is being served—enough to deter many people from eating it except at irregular intervals. Not only is the ultimate consumer receiving fish in a condition that fails to arouse enthusiasm, but the trade itself faces selling difficulties and heavy losses because of staleness and spoiling incurred by the present universal methods of shipping fish packed in ice in boxes and barrels.

There is a solution to this problem, and leaders in the trade have expressed their opinion that this solution lies in shipping fish in a frozen state. The drawback, however, is the public prejudice against frozen fish—a prejudice inspired by the unpalatability of fish frozen by the usual methods. Certain species—halibut and salmon, for instance—lend themselves admirably to freezing, but there are other species that, when thawed, taste like nothing at all. Experiments in brine freezing and with other methods have proved that fish can be frozen, kept for long periods in storage, shipped for considerable distances in adverse weather, and ultimately cooked and eaten without the consumer being able to detect any difference between frozen and strictly fresh fish. The success attending these experiments opens a new and desirable prospect for the fish trade, for freezing methods that will not change the composition of the fish or destroy its palatability make it possible for us to store fish when they are being caught, thus insuring a more even distribution later at a more even price. It means that we can ship greater distances, reduce the heavy losses in transit, and enlarge the market; it provides the retail dealer with a product that will not spoil before he can sell it; and the consumer will receive a piece of fish in a condition closely approximating that of freshly caught fish.

Some very bright minds in our industry are working along these lines. Personally, I believe that they are on the right track and that the future development of the fish trade rests upon giving the consumer a ready-to-cook boneless fish fillet with its quality and freshness maintained by shipment in a frozen state. In bringing about this desirable end the various governmental bodies, paternally interested in developing the fishing industries, can do much in stimulating action by the trade itself. There is a conservative element in our fishing industries that does not welcome innovations. "Get the fish on the market with the minimum of expense, labor, and trouble" is their motto, and this apathetic and stubborn attitude has not helped the business. In fact, it would almost appear that the adoption of modern methods in most cases was brought about by force, in the form of legislation, or by the competition of more progressive members. The Bureau of Fisheries and other organizations, by recognizing the absolute soundness of marketing fish in filleted and frozen form, are in a splendid position to convert the industry to this practice, and much more quickly than would be the case if the industry were left to discover this for itself.

Marketing of fish in fillet form involves numerous problems. The most important, perhaps, are those of labor and the profitable utilization of the waste. The labor problem is claiming the attention of many firms in the fillet business, and a way of mechanically cutting the fillets is being sought. Machinery is being used in some stages of the work, and one firm in Gloucester has evolved an ingenious series of machines which will virtually eliminate all hand labor in cutting the fillets. The Bureau of Fisheries might detail one of its officers to investigate labor-saving devices for fish cutting in use here

and abroad and publish its findings for the information of the industry at large.

Before tackling the problem of waste utilization I would like to commend the bureau's interest in the maintenance of the quality of fresh fish from the time it is caught to the time it is placed on the consumer's plate. A great deal happens to a perfectly good fish within that period. In many cases it is sorely abused by exposure to sun and air, from pitchforks, and through general rough handling. These things are responsible for heavy losses to fishermen and dealers, and also, to a great extent, for the public apathy toward fish as a food. I have yet to find the person who does not enjoy a piece of strictly fresh fish, well cooked; but the quality of much of the fish marketed nowadays is not appetizing. The Bureau of Fisheries is investigating this very important problem. The publication of its recommendations for the better handling of fresh fish is going to be very helpful to the industry.

The freezing of fish presents numerous problems. One of these (the discoloration of fresh haddock fillets in cold storage) has proved an obstacle to the expansion of the frozen-fillet trade. Harden F. Taylor made a special study of this matter in connection with his firm's work in that line and claims to have overcome the trouble. However, all fish dealers are not able to engage the services of scientists like Mr. Taylor, and they look to the Government bureaus for help and advice. Every man in the fish business would like to learn the best method for freezing fish and maintaining its quality without deterioration. Packing frozen fish for shipment presents another interesting field for investigation, and this, too, might be made the subject of a survey by the Bureau of Fisheries.

Taking up the second subject, viz, the utilization of inedible fish and fish waste, the present-day waste, not only of fish but of time and effort, in the fish business is appalling. Those of you here who have been to sea with the fishermen can testify to the enormous quantity of so-called "trash" and "gurry" that is thrown overboard. This waste occurs in almost every branch of the fisheries. It should be remembered that it costs just as much to catch a dogfish, monkfish, or skate at it does to catch a cod, haddock, or halibut, and it is the discarding of these inedible and unmarketable species that adds to the cost of the fish that are marketed.

Fishing can be made a really profitable business when everything that the fisherman takes in his nets or on his hooks can be taken to port and marketed. Many fine, edible fish are cast back into the sea because there is no market for them. This is not right. We must make a market for them. I believe that filleting is going to help in providing an outlet for the unpopular varieties, because filleting will conceal their idenity and they will gain favor from their flavor and not from their original appearance. Good, edible varieties, of repulsive appearance in the round (such as cusk, wolf fish, ling, skate, monkfish and others), skinned and filleted will hold their own in flavor and appearance with any of the popular varieties.

But that is something that will follow expansion of the trade in fillets. We have a real problem in trying to create ways and means for utilizing the inedible fish and the waste from dressed fish. Fish meal, fertilizers, glue, oil, and other products can be manufactured from such waste, but efforts to develop this angle of the fisheries have been somewhat slow. There are fish-reduction plants on the market, for most of which the claim is made that they can do the work. Experiments made with some of them, however, have proven that they are useful only with certain classes of fish. Other plants are too costly and complicated, while some are barred from many of the fishing centers because of the odor from them while in operation.

Here, again, is a field of investigation for Government bureaus. An investigation and report that will advise fishermen and vessel owners how to take care of the gurry and waste fish on board their vessels, and which will determine the kind of appartus suitable for the manufacture of fish waste into useful products and the possible market for such products, will be of inestimable value to the fishing industry. The necessity for action along this line is most urgent. The costs of operating a fishing vessel or a fishery are increasing every year. The unit of investment is becoming greater, but the price received for the fish is not keeping pace. The fishermen are not receiving an adequate return for their labor in many fisheries, and if nothing is done to make their work more remunerative we will have no fishermen. The native American dropped out of the fisheries long ago; the Canadians and Newfoundlanders

who took their places are also dropping out; and when the Europeans, who are taking their places, become Americanized, they will pass on also. Make fishing an attractive occupation financially and the young men of the seacoast towns and villages will turn away from the factories and take to the sea.

The vessel owner, as a rule, makes but little on his investment. Many fishing fleets show no profit from their fishing operations, but are maintained by wholesale producers for the purpose of insuring a supply of fish. Unless the full value is obtained from the fish caught—waste fish and offal included—a rise in the cost of the marketable product may be expected; and then fish will be so costly that it will become a luxury. By utilizing the waste and making a profit on it, the price of marketable fish to the consumer can be held at possibly a lower level than exists to-day.

The growth of the fillet business is creating a considerable tonnage of edible waste and bones. It is essential that some recovery be made here. Some companies are manufacturing flaked fish from the fillet cuttings, marketing the flakes in cans. These cuttings are being utilized also in the production of fish loaf. There is a field for investigations here, as the fillet waste is too valuable to be thrown into a glue factory or used as stock for cattle feed and for fertilizer. The bureau, I believe, has this problem in hand and is working upon it.

The bureau has many demands upon the time and knowledge of its skilled staff, and it is only natural that it conserve its efforts and confine its activities to problems worth while and for the good of the greatest number, and after a careful review of the subject upon which I was asked to speak, I believe I have presented the most important and the most pressing. Summed up briefly, it is (1) to help the industry give the public a bountiful supply of fish in the best possible condition and in the most attractive form, thereby expanding the market, and (2) to aid the fisherman to make the fullest use of our fishery resources by utilizing every pound of waste and thereby make his vocation more profitable.

Mr. Higgins. I think we all agree that Captain Wallace has pointed out a fruitful field of work. It is my understanding that the division of fishery industries is already engaged on problems of fishery technology that are particularly urgent. We should all bear in mind these problems of industry at the same time that we are carrying on our biological investigations, for if there were no economic problems, there would be no biological problems.

Captain Wallace. I noticed that in Mr. Sette's graphs of the cod and haddock fisheries he shows a considerable decline in the codfishery and a rise in the haddock. Would that not be from an economic cause? In the old days there was a great salt fishery conducted out of Gloucester and other New England ports. They wanted cod and would not take haddock. They brought them in for the dried salt codfish market. That business has practically passed out of existence. There is not the market for cod existing in the country to-day as there was years ago. Haddock has started to come up now. I wonder if there is such a thing as a decline in the actual quantity of the cod on the grounds. It would seem to me that there is still a large fleet of French fishermen coming across to the Grand Banks and taking codfish exclusively; also Portuguese. If you could get the statistics of what they are catching and compare them with those of about 20 years ago, you would be able to tell.

Mr. Sette. We don't know the cause exactly. The decline might be due to the failure of the salt-fish trade. I have been compiling statistics of the catch of foreign nations on the Grand Banks. I find that the Newfoundland catch, although it fluctuates, is now on relatively the same level that it was in 1850. It is interesting to note that they have an increased return per vessel and per man, probably indicating increased efficiency in fishing. It is up to the

investigator to separate the economic from the biological causes in the case of any change in the quantities of fish caught.

Mr. Radcliffe. The outstanding point revealed by Mr. Sette's discussion is the need for continuous, regular statistics for each year. If we had regular statistics, interpreted by economic conditions that we know about at the time, then we would have some idea where we were heading in this matter.

In 1921 the whaling interests stopped all operations on the Pacific coast. It was simply for economic reasons—there were just as many whales. The same factor applies to many of the fisheries that have come into prominence in later years. Prior to 1890 there was no demand for many of the fish that now are important. The fisherman did not bring them in. The economic factor is important.

Mr. Sette. When you really come down to the basis of this matter of yield of fisheries, the entire thing is economic. It is an economic impulse that causes men to fish for profit, and if investigators ignore the economic phase of the situation, they can't possibly explain what is happening.

Mr. Higgins. I have always felt that there was a great mass of information concerning fisheries that failed in getting over or producing results. I have not been able to decide whether it is because of the form of the investigation or solely because of the lack of an intermediary between the technical investigator and the fishery administrator—a go-between of some sort, an agency consciously developing and applying the results of theoretical research to fishery problems, to the actual formulation of regulations for the conservation or rehabilitation of a fishery. I believe the fishery investigators themselves necessarily must consider the final application of their investigations to the formulation of laws and regulations for the protection of fisheries, and in order that we may gain some more concrete notion of the problems of fishery administrators and of the actual application of fishery regulations I have asked Mr. N. B. Scofield, in charge of the department of commercial fisheries of the California Fish and Game Commission, to discuss some of these problems.

PROBLEMS OF THE FISHERIES ADMINISTRATOR

By N. B. Scofield

In charge commercial fisheries, California Fish and Game Commission

I can speak only as regards our experiences in California, some of which I will give with the hope that the problems that have arisen there and the way in which we have attempted to meet them will offer some benefit to those who may have similar problems to meet, or to those who are in a position to advise where the work of conserving the fisheries is in process of organization.

The first problem has to do with organization. At the present time the States have jurisdiction over their fisheries, and for that reason are responsible for the care and protection of these fisheries. The majority of the States have fish and game commissions, whose business it is to conserve the fish and game resources of the State. The tendency is to divide the work of conserving the commercial fisheries among separate commissions, and in a number of States we have one commission looking after the game and sport fish and another commission looking after the commercial fisheries.

I believe it is a mistake to separate the two interests, for the antipathy that exists between the sportsman and the commercial fishman is thus intensified. In a State where the two factions are interested in the same species of fish

it would be better to have only one commission as the arbiter between the warring factions, for with such an arrangement it is easier to come to an agreement. From the standpoint of economy in administration, in the enforcement of laws, and the operation of fish hatcheries, it is better to keep them together. Where there is a separate commission to administer the commercial fisheries there is a tendency to put in commissioners who are financially interested in the fisheries, for it is difficult to get any one to give his time who knows much about the fisheries and is able but who is not financially interested in some way. The general public knows very little about the commercial fisheries, especially the ocean fisheries, and for that reason is little interested in fisheries conservation. The result is that the commercial interests will dictate the policies of the commission; and it is a notorious fact that almost everyone is strong for conservation except when it hits his purse or his sport.

Many more people are interested in angling for sport, and they are strongly in favor of protection for all fish, especially if it is to protect them against the commercial fisherman. These ardent anglers frequently can be very helpful in having passed legislation to prevent the overexploitation of a fishery.

In California, you have probably guessed, we have one commission to adminster the State's work on game and fish, including the commercial fisheries. The California commission was established in 1872 and was at first known as the California Fish Commission. It is evident that in the early days of the commission it was as much interested in the commercial species of fish as in any others. Its first act was to bring the shad into the State, and the shad is certainly a commercial fish and not a sportsmen's fish. Next, a hatchery for the chinook salmon was established, a fish that is mainly commercial in California. Next, the striped bass was introduced and thrived amazingly. It is a splendid fish for both food and sport, and for that reason is the cause of most of the quarrels between our sportsmen and commercial fishermen. As time went on and the commercial fisheries remained comparatively unimportant, the commission became more a sportsmen's organization, with sportsmen as commissioners.

It was not until 1897, more than 30 years after the establishment of the commission, that any attempt was made to carry on scientific investigations in connection with the commercial fisheries. At that time investigations were begun on the life history of the chinook salmon. From time to time after that zoologists were employed to investigate certain problems pressing for solution. These were along the line of determining facts concerning the life history of certain species of economic importance. These included salmon, striped bass, crab, spiny lobster, abalone, and Pismo clam.

While these investigations were in no case continuous or complete, and there was no organized program for the care of the various fisheries, the commission made remarkably good use of the information gained from the investigations in getting conservation laws passed by the legislature.

With the birth of the tuna-canning industry in southern California in 1912, there also arose an interest in the fisheries on the part of prospective investors and others, and as a result of this interest numerous technical questions were asked of the fish and game commission, which for a time it tried to answer by referring them to persons outside of the commission. It became evident to the commission that it needed to have some one steadily employed who could handle the correspondence relating to the fisheries—one who could familiarize himself with the problems of the growing fisheries.

In 1914 I accepted this position and was given an assistant, and a little later we were given a room with a telephone, and later we acquired a stenographer, and soon our own files and reference books. We grew to be looked upon as a part of the commission. To some we were an unnecessary part and were jokingly dubbed the "hot-air department."

I have given these details to show that the organized fisheries work of California was forced upon it by the growing fisheries industries. The two members of the department busied themselves with getting all the information they could on the fisheries subjects that were expected to come up at the 1915 session of the State legislature. During that session, the two members of the new department were present at all of the meetings of the fish and game committees of both the senate and house. We were there at the request of the chairmen of the two committees to give any technical information that might be desired by the committees. It was easier in those days, when we were hitting only the high spots, to answer the questions than it was later, when we knew more about it. With all that, our information was of a higher order

than the committees were accustomed to getting, and that is not saying very much.

The fisheries laws, of California at least, are highly complicated, and the committees are glad to have State men with technical fisheries knowledge, as well as knowledge of game, continually present at the sessions, and the amount of work they turn over to these men would surprise you. Committee members will get technical letters from their constituents which they are unable to answer. They need advice as to how certain measures will appeal to their constituents. At all times we have been careful not to appear partisan, except, possibly, on the side of conservation. We are careful to give all the facts as we know them, so that the committee may draw its own conclusions. But, usually, members of the committee wish to know what our personal opinion is. The committees have more and more come to rely on the representatives of the commission, and there is no denying that the opinions of those representatives carry a great deal of weight. What we have done in this respect in California I think any State can do. These men, acting for the fish and game commission at Sacramento, are not politicians, nor are they lobbyists in the usual accepted sense. By this method most of the absurdities in the fisheries laws have been eliminated and good, protective measures have been adopted.

The law under which our commission was established was antiquated and did not meet the new conditions. Under the old law creating the commission the expenditure of money for scientific investigations could not be justified, and I believe this fault can be found in most of the old laws creating fish commissions and probably in some of the new ones as well. We presented a bill to the legislature which made it the duty of the commission to carry on investigations and to determine what measures are advisable for the conservation of any fishery. This bill passed without any opposition.

I do not see how any State fisheries department can get very far unless it has influence at the State capitol. I believe any fisheries department can gain the necessary influence by keeping men in attendance at the legislature who are honest and have tact and are scientifically trained. They should know the fisheries, of course. The advice and help of a lobbyist is a valuable asset to any State commission. That, I believe, will hold true for a Federal department or bureau.

To look back now over the past history of the California department of commercial fisheries it appears that chance was the controlling factor. At the very beginning we had a vague idea that the basis of our fisheries work must be accurate and complete statistics. We got a bill through during the 1915 session of the legislature that required fish dealers and canners to give the commission a monthly record of all fish received. This gave way to a much better law four years later which gives to the commission a carbon copy of every receipt issued to fishermen and which contain all the data desired. The same law required every fishing boat to register and to give a description of the boat and the gear used.

At the second legislative session after our organization we got through a fisheries tax which, together with the commercial fishing license already in existence, gave us adequate funds to enlarge the department.

We had the support of most of the fisheries people in these measures. The southern California tuna canners were for us. It so happened that the tuna fishermen were hard pressed to find bait—sardines or anchovies—and about the only place where the bait could be found was in the water around Catalina Island, which the sportsmen had succeeded a few years before in closing against the use of nets. The tuna canners wanted part of the island open for bait fishing, and they wanted and needed our help.

In the summer of 1915, while the Pacific division of the American association was meeting at San Diego, an afternoon was set aside for some papers and a discussion of tuna problems. One of the canners stated there that the tuna canners would like to know from the scientists how much fishing the albacore could stand without exhausting the supply. He said that the schools of albacore appeared to be getting smaller, although the fishery was then only two or three years old. Each year additional canneries were being built and the old canneries were being increased in size. He asked the very practical question: "Is there any way to tell how many albacore can be taken each year without ruining the supply?" If the canner could know that, he said, then he and others would know whether they should increase their investment

or decrease it. Nobody had the answer and no one seemed to have a very clear idea of how to go to work to find the answer. It struck me that it would be a good thing if our embryo department were to set itself the task of working toward the answer of this interesting and practical albacore problem.

Later, when I read W. F. Thompson's paper on the Pacific halibut, where he answered that very question, in part, for the halibut, I made up my mind we needed him for the albacore work. Our success in getting the fisheries tax bill through made it possible for us to get Thompson in 1917. Under his direction, a scientific fisheries program was built, the basis of which was to find out how much fishing a fishery can stand.

After the employment of Thompson, and after we had used unsuitable quarters in southern California for carrying on the albacore work, we came to see that any adequate fisheries program should be a continuous one, and that we should have quarters of our own. The commission, it happened, had the money, so we built a State fisheries laboratory at Terminal Island, near the canneries of that district and near Fish Harbor.

This laboratory stands as a symbol of the scientific fisheries program that has been instituted, and it is believed that it will aid greatly in making the program a permanent one. Our great difficulty was to get men for our work. Mr. Thompson, as director of the laboratory, had to search the universities for likely young men with zoological training, and these men he had to train in the technique required in the fisheries work.

A serious problem at this stage was the matter of salaries. We were a part of an organization that paid very poor salaries. It was hard to get the salaries raised above those being paid in the departments of patrol and fish culture, and before we were able to convince the commissioners, the executive officer, the State civil service commission, and the State board of control that these fishery investigators should get at least as much as a teacher of science in a high school, we had lost nearly all our men. But this we have not regretted so much, for the fisheries program that we have adopted is a sort of religion with us, and wherever those men have gone they have been spreading the gospel.

The matter of salaries has now been largely overcome. About all that remains for us to do is to develop another lot of enthusiastic investigators and trust that we can keep them. Possibly by that time the laboratory will have become famous as the producer of those young men who have already gone forth, and we will be besieged by young zoologist applicants who are up in their mathematics and who delight in statistics, adding machines, rapid calculators, and the rest of the modern improvements.

I have been asked by that department of the National Research Council that has to do with States relations how we managed in California to sell our scientific work to the fisheries people. That is not so easy to answer.

It is of advantage to the fisheries interests to have a State department that has sufficient knowledge of their business to help shield them from ill-advised legislation. They recognize the scientific work as a method of obtaining at least part of the evidence that will prevent unnecessary restrictions being placed on the fisheries. They feel that they should be interested in the scientific work, for they are paying the bill. We did our best to get them, or some of them, to understand our work, but with little success. They were unable to judge as to whether or not we were making the proper use of the tax they were paying. At one time they contended that they could not see how our scientific work was benefiting them one nickel, but when they were told by a person on whom they looked as an authority that our work was of a high order they were satisfied that we were all right. Later, when our work was in danger of being curtailed by a governor just elected on an economy platform, the fisheries people were sure they could not exist without the scientific work.

At this time we had received but scant encouragement from zoologists and scientific men within the State, so we were surprised and delighted at the splendid letters these men wrote the governor about our work, in response to our appeal. From that time on the governor's office, as well as the members of the State board of control, took a greater interest in us, and it raised our standing within our own commission. People like you better after they have done something for you, and this holds with scientific people just as with anyone else.

One of the biggest of our department problems has been the enforcement of the State "fish reduction act," which would regulate the use of sardines for

reduction purposes. This law was put through several years ago at the request of the sardine canners, who were alarmed at the wholesale destruction of sardines in reduction plants. Each cannery has its by-products plant, and when, after the armistice, the sardine market collapsed and the market for fish oil and meal remained high, the canners naturally wished to tide over the depression by using a liberal amount of sardines in their reduction plants. A law was put through by agreement between the fish and game commission and the canners which permitted the canners to use 25 per cent of the sardines for the manufacture of oil and meal At first we had difficulty in preventing the canners from using more for reduction than the law allowed, but as we gained in experience and understanding of the intricate ways of the sardine-canning business we were better able to hold them down. We had many legal battles, all quite friendly ones, and succeeded in adding some very interesting court decisions to the State's fisheries history. The law was amended several times and will be changed again at this present session of the legislature. The enforcement of this act, together with all the conferences and correspondence, takes a great deal of our time and energy. We hope to get a law so simple this time that there will be no argument about it.

One of the latest things we have had to contend with is the floating reduction plant which purposed to operate outside the 3-mile limit and receive fish caught outside the 3-mile limit. The first one of these plants, a large concrete ship, was anchored about 4 miles from shore, but within Monterey Bay. When two purse-seine boats delivered sardines (some 90 tons) to this plant, we placed the fishermen under arrest, seized the two nets, valued at $4,000 each, and got a temporary injunction from the superior court of Santa Cruz County to prevent the company from operating the plant. They went to the Federal court for an injunction and damages against the commission and its officers. The Federal court decided that Monterey Bay was State territory. The company then appealed to the State supreme court and we are still awaiting a decision from that court. In the meantime, this plant moved down the coast and anchored off Santa Barbara, and at about the same time another and larger floating reduction plant, which had outfitted at Oakland, moved down to the same place after losing a suit in the superior court of Alameda County to enjoin the commission from interfering with their operations. With our patrol boats constantly on guard the fishermen were afraid to fish for these plants and one of them has given up and returned to port. The other is again asking for an injunction in the Los Angeles courts.

This illustrates a very interesting situation. The State's jurisdiction ends at the 3-mile limit, except possibly in bays in California. In some of our fisheries all of the fish are caught outside the 3-mile limit. In others, a large proportion are caught outside of that limit. Seventy-five per cent of the salmon caught by trolling off the coast of California are from outside the 3-mile limit. We have suspected that many of the salmon caught off the northern coast of California are from Oregon and Washington. Recent tagging experiments show that some of the salmon from the Sacremento River are caught by trollers off the coast of British Columbia. California fishermen go into the waters of Mexico for a part of their fish, on which Mexico attempts to collect an exhorbitant tax. Our problems have grown until they extend beyond the jurisdiction of the State.

This situation can be met in part by international treaties through the aid of our own Government, but we need also some sort of cooperation between States, as well—some plan whereby the States can look after their own local affairs but be under some sort of supervision by the central Government so that there will be coordinated control of the fisheries common to several States. Under the international fisheries treaty between the United States and Mexico we are trying to work out a plan under which the two Governments and the State of California can cooperate in the case of California's fisheries. Possibly this will work out as an example of how the State and the Government can cooperate.

Mr. Higgins. I should like to ask Mr. Scofield if he considers the plan of fishery administration, as worked out in California, applicable to other States on the Pacific and Atlantic coasts, and if the urge to conduct the scientific work necessary to put such a system into operation can come from the outside, such as from the Federal Government, the Bureau of Fisheries, or if it must come from the industries within these States?

Mr. Scofield. I think that that could well be started by the Federal Government or its agents. People in the States don't know, usually, that they need an organization of that sort, and if it is suggested directly, it seems to me they can be interested and should be able to get those in authority, for instance a fish and game commission, to act together with the industry and form a department. It seems to me it could be developed in the same way in which we proceeded in California.

Mr. Higgins. You think the Bureau of Fisheries could take a leading part in developing adequate administrations in the States?

Mr. Scofield. I think so. It would have been a great help to us.

Mr. Radcliffe. Isn't it virtually necessary that you have some clearing house of this kind? Your fish do not recognize boundary lines. Therefore, if you are going to have proper statistics, shouldn't there be some way of getting the several States to provide comparable ways of collecting statistics, such as a central clearing house?

Mr. Scofield. I think that would have to be done to get the statistics right.

Mr. Higgins. We all appreciate Mr. Scofield's review of the experience of California. California, I think I can say, has led the United States in wise fishery administration, and we can look to California as a model for all the other States in the matter of obtaining exact knowledge of the course of a fishery, through their excellent system of taking statistics.

I have something here that will prove of interest—an article by a rather caustic critic of the Ministry of Marine and Fisheries in England, whose criticisms are always interesting and sometimes very much to the point. He writes under the name of "Quibbon" in the Fish Trades Gazette in London.

There has just been issued, as an appendix to the report of the U. S. Commissioner of Fisheries for 1926, a modest pamphlet, of 36 pages, on "Progress in Biological Inquiries, 1925," by Dr. Willis H. Rich, the assistant in charge of scientific inquiry. The price of this paper is 10 cents, or fivepence (from the Superintendent of Documents, Government Printing Office, Washington, D. C.), which contracts with the price of similar publications issued by our stationery office. A perusal of it shows how enormously the United States has advanced in fisheries research in the last few years. Not so very long ago there was very little organized research, although many papers were published dealing with certain fishes or fisheries. After reading this report, it would appear that fishery research in America is now as intense and extensive as it is in Europe. Almost every field of inquiry is covered in a systematic way, and the methods which have been proved of utility are now adopted. The report is an able summary of what is being done. It is said that "the policy of stressing the study of the immediate rather than the ultimate causes of fluctuations in the abundance of fish of each species was continued"; and that "emphasis has been placed on the investigations giving the greatest promises of results of practical importance in the conservation and development of the fishery resources." This is as it should be. It is the duty of fishery departments everywhere to follow the same policy, leaving it to the universities, or other institutions (as the Marine Biological Association in this country), with more or less financial assistance from the State, to undertake the more theoretical researches—which, however, it must be said, may be ultimately of no less practical importance. It is pointed out that true conservation of the fisheries means not only guarding them against depletion, but making use of them to the greatest possible extent compatible with their perpetuation. The difficulty is to determine from year to year what the excess production is, and how much of the stock may be taken by man without endangering the future supply. We must know the causes of the fluctuations in the yield from year to year, and how these fluctuations may

be controlled, if that be possible. The greatest difficulty the bureau has to contend with is the scarcity of properly trained men interested in fishery investigations. * * *.

Just what course scientific investigations of the fisheries should follow is a problem of greatest concern to all of us. I have asked Mr. Will F. Thompson, who has had many years of experience in fishery investigations of a rather technical type, to give us his ideas of what scientific investigations of the fisheries might well consist.

SCIENTIFIC INVESTIGATION OF MARINE FISHERIES

By Will F. Thompson

Director of scientific investigations, International Fishery Commission, United States and Canada

It is generally accepted that the end and aim of governmental fisheries science is the conservation and the more adequate use of our fisheries. Under this, the detection and prevention of overfishing comes first, as far as the sea is concerned, for by far the greater number of our fishery industries are based upon species not capable of artificial propagation or culture of any kind. Next in order of importance comes the detection and foretelling of the great fluctuations in abundance due to natural causes, such as unusually abundant year classes. You will hear enough with respect to the latter before this conference is over. My purpose is to emphasize to you the great problems underlying the first great objective—the detection and prevention of overfishing.

I wish to do this because I am firmly convinced that they are not only really great problems, from an economic standpoint, but also truly great problems to the biologist. The men engaged upon these major problems are not confined in their work to the economic application of principles already discovered; they are formulating those principles and attacking problems in which the investigator in pure biology should be proud to be interested.

The fundamentals underlying the utilization of our sea fisheries have not yet attracted the attention they should. As one looks through the literature, it would appear as though the basic rule for the conduct of investigations has been the general one that everything connected with the fish or its environment, the ocean, is of importance and should be studied. This view is undoubtedly sound, but it is equivalent to saying that the study of everything on land is of importance when studying the production of beef cattle. The statement is too diffuse to have any meaning. It has led, in the case of the North Sea fisheries, to the comparative neglect of a really great problem—that of the nature of overfishing.

To say that our problem, or group of problems, is the study of the decline in productivity of certain fisheries is trite. That definition is commonly accepted without any thought as to its real significance. Suppose that we attempt to probe a little deeper into the yield of a species of fish. We find very shortly that we do not know what overfishing really consists in, that we have no measure of the strain a species will stand, that we do not know the character or type of the reaction of the supply to the demand. Our ignorance is intolerable in view of the greatness of the stake, and the presence of constantly decreasing yields for the effort involved should stir us to some sort of action. Regardless of theory, the abundance of fish must not go on decreasing, especially in the face of the fact that we are not at the crest of a period of exploitation but rather at the beginning of such an era

This era of exploitation is accompanied by an era of constantly increasing efficiency, sufficient to mask for the time being whatever decline may really exist. One is tempted to date the beginning of intensive exploitation as far back as the application of steam to marine vessels and to railway transportation; yet since that time new apparatus, new internal-combustion engines, new methods of handling have been devised, so that we find ourselves under the constant necessity of redefining what we mean by an intensive fishery.

I am thinking specifically of such fisheries as those for the halibut, the plaice, and the albacore. The total yield may remain the same, yet when some accurate measure of fishing effort is used, it is seen that a far smaller catch is obtained for that effort and a far greater area is covered. One sees a con-

stantly increasing intensity necessary to maintain the yield. With regard to other species, what reason have we to rely upon the belief that the most prolific and as yet untouched species will not follow in the path of the halibut and the plaice, in the face of the infinite threat of mankind's expanding powers?

I will use the halibut to illustrate my theme, as I am most familiar with it. The details of the decline in catch per skate of 210 hooks have not been worked out fully, but in so far as they have, the curve of abundance may be shown tentatively in Figure 12, where the decline is represented on a logarithmic scale to show that it is taking place at a steady rate of fall. The straight line, representing the rate of fall, is translated in Figure 13 into actual poundage per skate of 210 hooks to give a hypothetical curve of abundance.

In any subsequent remarks concerning abundance you will understand that by abundance I mean this yield per skate of 210 hooks. This is the most accurate measure of abundance we find at hand.

This decline represents only the older section of the banks, which constitutes now a more or less distinct fishery. The total yield from this has declined greatly in recent years; just how fast we do not yet know, but we may draw in a level line in Figure 13, representing the total yield as shown, for the purposes of discussion. Nor do we as yet know the intensity of the fishery from year to year, but we may calculate, from the yield per skate, what it must be to maintain the total yield, and we represent its increase by a tentative line, rising rapidly. In general our work supports these curves, as they would be modified by a declining yield.

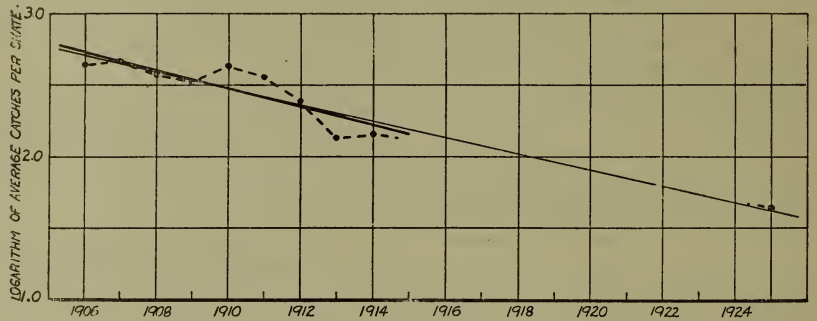

FIG. 12.—Rate of decline of halibut catch per unit of gear, represented on a logarithmic scale to show the steady rate of fall. Short straight line shows rate of fall as determined from data for 1906 to 1914, inclusive. Long straight line shows rate of fall as determined after addition of data for 1925.

I would call your attention to a fact that all the trade knows—that the economic effort involved for the yield obtained has steadily risen, just as is shown by the increased number of skates in Figure 13. It would seem to me that in this is to be found one of the worst phases of the dreaded overfishing. The latter might be defined as that condition of a fishery in which the cost of operation is a maximum for the yield obtained. Let us, for the time being, forget the fears for the very existence of the fishery and think only of this economic condition. The cost of operation of this fishery is raised automatically to the highest point the traffic will bear. It is, in very fact, making the public pay as dearly as it possibly can for what it gets. A very slight change in prices or costs would be disastrous. That may be overfishing. Every improvement in gear, in engines, or in vessels enables the intensity to rise, as I have seen myself, repeatedly, in the halibut fishery in such cases as the adoption of small hooks. And it must automatically result in decreased abundance, as shown by yield per standard hook. The reopening of the war-closed North Sea automatically resulted, by this reasoning, in a resumption of the preceding level of abundance as determined by economic factors, not necessarily by biological factors. It does not seem to me necessary to seek a biological explanation for this seemingly more stable lower level.

I have made this statement most emphatic for the purpose of clarity. In reality, we do not know enough about the relationship of these curves to discuss them intelligently. There are statistical, biological, and economic phases to this great problem, but in that equation, or series of equations, which

will link these three graphs—of intensity, abundance, and yield—lies the problem of fisheries science as I see it.

It seems to me, from my experience with the halibut and my reading in fisheries reports, that it is possible to maintain the abundance at a higher level by restricting the fishery. Thus, the yield per skate on our Pacific coast is directly correlated with the cost of operations. Banks as far away as Kodiak Island yield now about 120 pounds per skate; near Cape Ommaney about 60; and in Hecate Straits 43. Of course, these areas may be on a continuous decline, but the rate of the decline is determined largely by economic conditions, and possibly it might be stopped easily. The question then arises, what might the yield be were the decline stopped at any particular level? Would it be noticeably less at the higher levels of abundance? It might be greater.

We must at once admit that there may be a remote possibility that a fixed level of abundance (say at 40 pounds to a skate) would remain at that level even though 40,000,000 pounds were taken out by a highly intensified fishery of 1,000,000 skates yearly; whereas a fixed level at 120 pounds to a skate might yield, without decline, about 20,000,000 as the result of the use of but one-sixth of a million skates. The difference in total yield of 20,000,000 might,

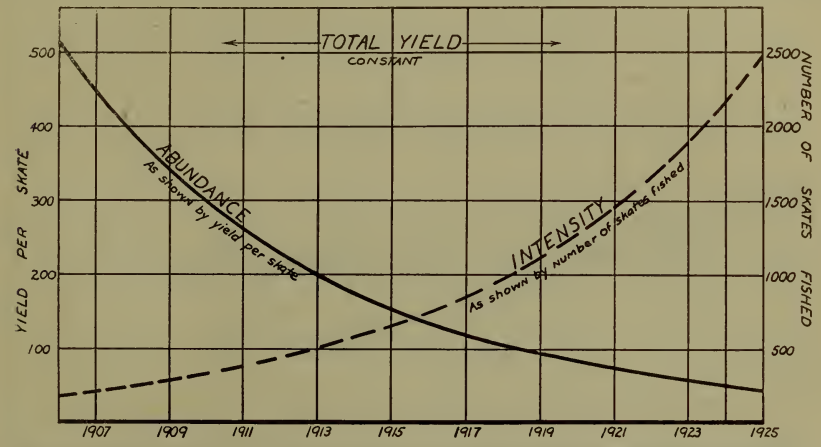

FIG. 13.—Hypothetical abundance in halibut fishery, showing various levels (left margin and unbroken line) and the numbers of skates required (right margin and broken line) to produce a fixed annual yield of 100,000 pounds

from an economic standpoint, justify the principal of making the public pay all it could. Would it be economically worth while to double the yield by fishing six times as much gear? Perhaps so; perhaps not.

	Case 1	Case 2
Abundance	40 pounds per skate	120 pounds per skate.
Total yield	40,000,000 pounds	20,000,000 pounds.
Intensity	1,000,000 skates	166,667 skates.

However, we have no such knowledge as to the course of events. The total yield might very well be the same in both cases, or even greater under restriction. Nor do we know what the final reaction of a species is to intensified fishing. That is a biological problem of first magnitude to which we will return.

As we have said, the great fisheries have been and are in a condition of constant increase of efficiency and intensity. This increase has been so consistent and continuous as to give ground for thought as to whether it will reach a limit, or as to what a terrific strain it may finally impose on our available species.

The total yield, however, has not increased correspondingly in the threatened fisheries, such as plaice and halibut, however much it may have in those

formerly less used, or however much the yield may be affected by extension of banks. The question arises in my mind as to the result were this increase of efficiency and intensity to stop. Would the total yield thereupon enter a decline? It would seem to be doing so in the case of the halibut on the older grounds despite the increase in efficiency.

But speculation is rather useless as to the future of our great fisheries—we have no real records as yet to use. We must observe what fisheries we have before us, and we must recognize, in the *continuity* of the decline in abundance, a very real threat. We must never forget that there may be a lower limit of abundance beyond which the species may not be able to breed at all; that if there is no limit to efficiency and intensity there is no limit to the decline. And the whole vast future of our fisheries may proceed on a grossly wasteful and inefficient, as well as dangerous, course if we do not solve our problem. Nor will conservation ever be sane until based scientifically.

I might briefly summarize the points I wish to make, and then take them up in turn for discussion.

1. Our problem, as reviewed above, requires, first of all, adequate studies of yield, not merely total but per unit of gear or effort.

2. It then requires a study of the reaction of the species to fishing.

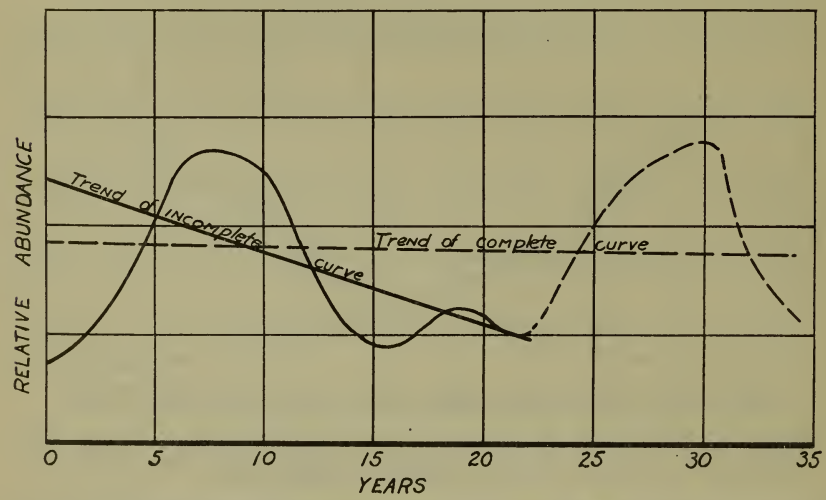

FIG. 14.—Hypothetical fluctuating abundance of a species of fish, with mathematically determined trends to show error that might arise from incomplete data

3. And, finally, our proposal to restore previous levels of abundance will require restrictive measures based on the biology of the species and the economics of the fishery.

The scientific investigation of marine fisheries has come to mean, to me, first of all, the establishment of adequate statistics of yield and abundance. This is not a simple problem. It requires accuracy, of course, but it also requires the discovery of some measure of abundance—as the yield per skate of 210 hooks in the halibut fishery. It implies, still more, the careful observation of the unit of gear for small changes, such as the use of small hooks instead of large, and the recording of the exact localities. But over and beyond this, it implies the study of the categories of fish taken, changes in which accompany and may exceed in value, to us, the changes in quantity. The study of these changes by means of age analyses, etc., is, of course, biology. How it can be separated from statistics is unknown to me.

Methods of sampling the catch are still in rudimentary shape. Once the data are at hand, there arise the difficulties of interpretation, and the scientific point of view becomes of exceeding value. To cite but one case as a warning: We have found in the herringlike fishes great fluctuations in abundance. These extend over a period of 10 years, perhaps. The result to our curve of abundance might possibly be thus (fig. 14):

Even to the nonmathematical eye it should be plain that the trend can not be determined until a great many years' records are available. In such fisheries, detection of a decline in abundance or in total yield may require more time than has elapsed since the beginning of the real fishery.

It seems, finally, that the separation of economic effects is difficult and occasionally impossible.

The biological problems involved in the study of the reaction of the species to intensive fishing revolve for us very largely around the possibility of greater resistance at lower levels of abundance. To illustrate: A highly respected English scientist postulates[5] that the more intense the fishery, the better condition it finds itself in. This reasoning is based on the sequence in events in the North Sea as a result of its closure and is, very briefly, a theory that under natural conditions the competition between adults and young starves the latter and vastly reduces their number and their rate of growth. When the banks are intensively fished, the removal of the adults allows the young to grow faster and survive in greater numbers, so that they replace all that the fishery takes. Unfortunately, it is impossible for us to ignore selection by the fishermen when both young and adults are available, and facts from other sources[6] indicate that as a result of the North Sea closure both young and adults increased in numbers. Furthermore, the young occupy "nursery areas." They do not inevitably compete with the adults, nor are the great mass of their competitors for food necessarily members of their own species. But I am not criticizing the paper; it is theory I am interested in, and Garstang's theory does at least present the possibility that an intensive fishery leads to a greater productivity at lower levels of abundance. He makes use of facts of growth, and should make use of facts of migration, to formulate his equation, for such it really is.

Another attempt is equally interesting. A Russian scientist[7] postulates that the *production of a surplus pound* of fish *requires thrice the nutriment that the maintenance* of a pound requires. The abundance of fish, under his theory, will fall until sufficient food supply is freed for use, so that the rate of reproduction (beyond that to maintain the stock existing each year) exceeds the demand by man. It is hard to understand, at least for me, but he does make an effort to expresss the problem of the increased resistance or increased productivity of a species, which he assumes exists at a low level of abundance. He believes the food supply to be definitely limited in amount and used largely by the plaice, as does Garstang, but fails to see that the rule he applies to the plaice might well apply equally to the animals it uses as food. The intensive use of the food supply by the plaice might lead to greater productivity of the said foods and the elimination of useless competitors, so that in the end it might not make any difference how many plaice existed—they would never run short of food, any more than mankind would run short of plaice.

However, the battle is fairly joined, as to the productivity of a species under various intensities of fishing. To me the only possible method of solution is the experimental one. But it is a great problem, indeed, and in its solution lies our sole hope of reducing the exploitation of our major marine resources to a logical basis.

In passing, I would like to call your attention to the biological importance of such studies. Mankind's demand on the fisheries is a type of adverse condition leading to greater mortality, very similar in many of its effects, perhaps, to the adverse conditions met by a species in nature. The power of survival exhibited by a species in contact with man seems to me to be that power that has enabled it to survive through the ages; and the record of evolution may be, in fact, the record of development of these powers.

Our analysis of the factors important to us may throw light upon the nature and methods of evolution.

Let us consider for a minute the facts brought to light in the study of dominant year classes in herring, sardines, and other fish. It might be supposed that the abundance from year to year would remain level; but our experience with sardines, herring, and Mr. Sette's mackerel, as well as locusts

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[5] Garstang, Walter: Plaice in the North Sea. London Times, Apr. 21, p. 15, and Apr. 26, p. 20; 1926.

[6] Heincke and Mielck: Schongebiete für die Scholle in der Nordsee. Berichte der Deutschen Wissenschaftlichen Kommission für Meeresforschung, neue Folge, Band 11, Heft 1.

[7] Baranov: On the question of the dynamics of the fishing industry. Bulletin of Fishery Economics, No. 8, Moscow, 1925.

and other animals, has shown this to be far from true. There very obviously is great variability in the conditions of survival, the yield varying widely from year to year, even in cultivated organisms. We have learned, indeed, that an organism must meet, in the course of its existence, periods of varying length in which adverse conditions of varying intensity prevail, causing great unequal infant mortality.

A species in the state of nature has existed throughout the ages. It has had to survive the most extreme of these periods in length and intensity. It is my thought that this variability in nature has played a major rôle in evolution and in the survival of species. Many characteristics of a living organism are of importance from this standpoint, and theoretically, at least, it is easy to conceive that the reproductive power of a species must have an excess beyond that required by a favorable or normal year.

I think of many characters of a species as adapted to meet the normal or ordinary emergencies of the annual cycle. The power of resisting freezing, which many spores have, is an example. These many characters must be developed far beyond the ordinary in order to meet the occasional extraordinary conditions. For our purposes, however, I am thinking mainly of those characters that serve to bridge the adverse periods of various durations. An animal, developing warmth of blood, is capable of surviving more than a year with less loss in numbers. So, too, the young are protected by the adults through the period of highest fatality, the protection in some cases lasting sufficiently long to bridge an ordinary long period of difficult years.

Foremost among survival characters one must place age. The halibut reaches a great age, as do the elephant and man. One inevitably thinks of the significance of this age as a survival factor of first magnitude. The loss of one year's spawning is but a fifteenth of the total loss for the life of an individual, and after a period of adverse years the adult is left to spawn anew.

But species vary widely in such characters as food, habits, age, fecundity or egg production—they meet the situation differently. Why can not we expect infinite variety in the reaction of survival characters to the strain imposed by man? The halibut is long-lived and comes to maturity at 12 years. Man's attack on the adults shortens greatly the average duration of life and seems to me to be undermining the very character upon which the halibut largely depends for its power of survival. May his fishing efforts not result in the inability of that species to meet and overcome some prolonged period of adversity? In that case it may disappear, entirely or over large areas. The reverse may be true of the herring—who knows? But at all events, it may be fair to think that we will have as many problems as there are species.

Perhaps, using the factors of age, food production, egg production, and migration, man may be able at some time to deduce some general law as to the productivity of a species and as to the value of characters contributing to this power of survival. The solution of such a problem may contribute vitally not merely to fisheries science but to biology. The power of survival is an essential part of the evolution of a species.

The most promising method of attack seems to me to be through experiment, carefully observed by statistical method. Under known biological conditions of age, migration, spawning, etc., certain results may be obtained. In time a general law may be framed to bring these into harmony, but until then, and as a means to such an end, we certainly must seek results experimentally.

So much for the second great problem of fisheries science. Our first was to obtain accurate methods of observation, our second to ascertain the method of reaction of the species to the strain. Our third must be to devise methods of protection.

After much thought concerning the halibut and other species, it seems to me that almost all protective measures are based upon the degree of migration existing. The reason for this is easy to see when the significance of a high degree of independence between banks is grasped. An isolated race of fish is depleted as a separate unit and must be rebuilt as a separate unit. With a homogenous, freely moving species the case must be just the reverse—it must be dealt with as a whole. Let us, then, set the study of migrations above all others when protective measures are discussed.

It is not necessary to review at length the means of such study. Tagging, the finding of racial characters, of differing growth rates, of finding physical traces in the individual of its past life history—all these are biological studies of great importance.

My purpose is not to emphasize to you the known methods of research. I wish, rather, to outline the great problems; and in regard to the nature of protective measures I find one of these great problems.

To begin with, if restriction is necessary, there are many types of it. Some species have been protected by saving the spawning adults, others by saving the immature. There is no cold scientific reasoning back of these measures. They are restrictive, they are expedient from an economic standpoint, they are popular, and hence they are those that it is advisable to impose. Once restriction is granted as necessary, and as either limited in area or general in application, the choice of restrictive measures becomes economic and political, not biological. I am not speaking of the salmon, of course, in which there is but one stage to restrict, but of marine fishes, many life stages of which are exploited. Restriction and regulation, however necessary, are not and can not as yet be based upon biological reasoning.

To illustrate, is there any biological reasoning that justifies the protection of halibut young? They are the most abundant members of the species. They are scarcely touched as yet. Many of those taken would die before maturity anyway. As a matter of fact, however, an economic reason justifies their protection—they are not worth as much for food, pound for pound.

I wish to suggest here, as a biologist, that the existing relative abundance of the various stages or ages of fish in nature is presumably that best fitted for the survival of a species. That is, if a species spawns 1,000,000 eggs, that habit has been acquired by a natural selection to meet the conditions of the environment. So with the young—their numbers are adapted to conditions to be met. But somewhere in the cycle between egg and adult must be a stage or stages in which there exists the reserve power of survival. Until we know, then, what characters affect this reserve power our restrictions and regulations may be more or less arbitrary.

There is only one sound biological principle that I can suggest. That is, that if mankind takes his toll before a period of great mortality, it must mean less to the species, proportionately, than if the toll were taken after such a period. That is, put concretely, if 1,000 pounds of fish exist, 100 pounds taken constitute only one-tenth; but if this 1,000 pounds pass through a period of mortality that reduces it to 500 pounds, and the toll then be taken, the toll of 100 pounds constitutes one-fifth instead of one-tenth. If to spawn and reproduce a species passes through such periods of intensified mortality, surely one would safely recommend that wherever possible man's toll be taken before these periods.

There is, of course, another thought—that where a species taken mostly as an adult declines in abundance it would be in order to protect the adult stage as the weakened link in the chain. But, of course, industry has its say there.

To sum up these remarks on methods of protection, I would like to think that I have impressed upon you the unscientific state of our present knowledge and the great need of earnest study of basic principles by biologists. In fact, some such remarks as these may apply to all three of the great problems I have touched upon—in determining the trend of the abundance there is need for careful study, in gauging the reaction of the species to the drain of our fisheries and to the results of protection the possibilities and principles are yet to be outlined, and in devising logical methods of protection the field of thought and discovery is wide open.

Looking back over that which I have thought and written, it seems to me that what stands out most clearly is the vast promise in this field of work for one desirous of thinking as he goes. The accomplishments in every essential phase of marine fisheries science seem to be almost *nil*—to have been sadly neglected, compared with what may some day be true. There is here vast opportunity awaiting some clear-minded thinker in biology; and in view of the uncertain knowledge of the basic necessities in marine fisheries conservation, no one taking that duty seriously can afford to expend his efforts aimlessly.

Mr. Higgins. Because I had the making of the plan, I took the liberty of preparing a paper myself, as a target for you to aim at, something to center our discussions on, something to carry in the back of our minds while we are going over each investigator's individual work—the policy of the Bureau of Fisheries in regard to biological investigations. I call them my own ideas; at least I

wrote them down, but you will recognize, no doubt, a familiarity in what I have to say. I invite criticism; I invite discussion; and being a target, your sharpest shafts will be welcome.

THE POLICY OF THE BUREAU OF FISHERIES WITH REGARD TO BIOLOGICAL INVESTIGATIONS

By Elmer Higgins
Assistant in charge, Division of Scientific Inquiry

The first step toward the conservation of our aquatic resources, taken because of the well-defined conviction as to their imminent destruction, was made by Congress in 1871, when the United States Fish Commission was established. Spencer F. Baird, the first commissioner, outlined the duties imposed by Congress, as follows:[8] That it should be the duty of the commissioner "to prosecute" the necessary inquiries, "with the view of ascertaining whether any and what diminution in the number of food fishes of the coast and lakes of the United States has taken place; and, if so, to what causes the same is due; and also whether any and what protection, prohibitory or precautionary measures should be adopted * * *." Thereupon, numerous activities, which had been conducted by scattered agencies such as the United States National Museum, the State fish commissions, and private agencies, were stimulated and centralized by the amazing energy of Commissioner Baird and his associates. The United States Fish Commission (later the United States Bureau of Fisheries) has been, therefore, since its inception, essentially an investigative advisory organization, whose duty it is to discover the need for and devise the means of practical conservation work. The principal direction in which the expansion of original plans has occurred is in the development of fish-cultural operations, intended to prevent depletion or build up the fisheries that have been depleted, and in the administration of the fisheries of Alaska, including the fur-seal industry.

Due to the rosy promises of the newly discovered art of fish culture, Congress was induced to add to the duties of the fish commissioner, at the end of the first year of his service, the artificial propagation of fishes. Under the influence of Professor Baird, an admirable program was initiated and carried out vigorously for many years. Investigations in marine biology were fostered and encouraged. A great marine biological station was established on the Atlantic coast. Vessels and equipment for deep-sea explorations were procured and many extensive expeditions to distant seas undertaken. Physical and economic inventories of the fisheries were made, and technological investigations looking to the more adequate utilization of fishery products and the developing of new fishing areas were begun. It was in the field of fish culture, however, that the most evident progress was made. The principal activity of this most versatile first commissioner was directed toward the wholesale replenishment of supposedly depleted waters. So successful was fish culture in the United States that in 1880 the grand prize of the International Fisheries Exposition at Berlin was awarded to Professor Baird "as the first fish-culturist in the world."

The early attitude of the commission toward scientific work, which included the "systematic investigation of the waters of the United States and the biological and physical problems which they present," was admirably expressed by G. Brown Goode[9] in 1884 as follows:

> The scientific studies of the commission are based upon a liberal and philosophical interpretation of the law. In making his original plans, the commissioner insisted that to study only the food fishes would be of little importance, and that useful conclusions must needs rest upon a broad foundation of investigations purely scientific in character. The life history of species of economic value should be understood from beginning to end, but no less requisite is it to know the histories of the animals and plants upon which they feed or upon which their food is nourished; the histories of their enemies and friends and the friends and foes of their enemies and friends, as well as the currents, temperatures, and other physical phenomena of the waters in relation to migration, reproduction, and growth. A necessary accompaniment to this division is the amassing of material for research to be stored in the National and other museums for future use.

[8] The status of the U. S. Fish Commission in 1884. By G. Brown Goode. Appendix E, Report, United States Fish Commissioner for 1884 (1996), p. 1141.
[9] Idem, p. 1141.

All of this, you will admit, has a distinctly modern flavor, but he concludes[10] that in order to neutralize destructive tendencies in the fisheries it is necessary to do three things:

1. To preserve fish waters, especially those inland, as nearly as it may be possible in their normal condition.
2. To prevent wasteful or immoderate fishing.
3. To put into practice the art of fish breeding: (*a*) To aid in maintaining a natural supply; (*b*) to repair the effects of past improvidences; and (*c*) to increase the supply beyond its natural limits rapidly enough to meet the necessities of a constantly increasing population.

The difference between present-day opinions and those announced a half century ago will be given more consideration later, but the failure to emphasize and to analyze carefully the fact of depletion and to determine and remove its causes, particularly in the great shore and marine fisheries, or to recognize and take advantage of the natural fluctuation in the abundance of the fish supply in the sea, may be understood readily and condoned when it is remembered that 50 years ago our Nation was still young, that untouched natural resources awaited exploitation and development, and that the extent of present-day needs, with the resulting strain on the fisheries, was undreamed of. Furthermore, the great promise and the popular success of fish culture induced a complacent confidence, as it was believed that the control of the fish supply was within easy grasp. This confidence may be illustrated by Goode's words: "Here the fish culturist comes in with the proposition 'that it is cheaper to make fish so plenty by artificial means, that every fisherman may take all he can catch, than to enforce a code of protection laws.' The salmon rivers of the Pacific slope," he continues, "and the shad rivers of the East and the whitefish fisheries of the lakes are now so thoroughly under control by the fish-culturist that it is doubtful if any one will venture to contradict his assertion. The question now is whether he can extend his domain to other species."

How well founded was his faith in the all-effectiveness of fish culture in maintaining or restoring the fisheries in the face of all possible destructive influences may be seen by the fate of the three great fisheries that he chose as illustrations. Despite 55 years of artificial propagation, the Pacific salmon fishery has declined alarmingly, the pack in Puget Sound[11] in 1924 amounting to but 12 per cent of the peak production in 1913. The shad fisheries of the east coast have declined, on the whole, 74 per cent from 1896 to 1923, and now are totally destroyed in many of our rivers.[12] The whitefish fisheries of the lakes, despite an annual distribution of 409,000,000 eggs and fry per year,[13] have declined in yield from first place in 1880 to fourth place in 1922, with a yield only slightly greater than that of suckers.

These statements should not be construed as an attack on fish culture in general, for the success and utility of the artificial propagation of many species is proved so fully that blame or praise from me is unnecessary. The entire system should not be condemned because the too optimistic hopes of its early advocates have not been realized; and if present methods fail to produce reasonably expected results, a measure of the blame should fall on the aquatic biologist who neglects this phase of fishery science and on the fishery administrator who accepts hatchery records uncritically as proof of the real abundance of fish.

While the early years of the United States Fish Commission may be characterized as the era of fish-cultural development, the liberal policy with regard to scientific research resulted in the production of a rich and varied literature dealing with many phases of marine and aquatic biology, in which surveys and explorations, with the cataloguing and description of animals new to science, were most prominent. The type of biology popular during the first three decades of the commission's work is indicated by the fact that 71 per cent of papers on the biology of fisheries (13 per cent of all papers), in the document series, were devoted to systematic ichthyology, and papers on other marine

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[10] Idem, p. 1148.

[11] Pacific Fisherman Year Book for 1925, p. 76.

[12] The total yield of the shad industry of the Atlantic coast, from records of the U. S. Bureau of Fisheries: 1880, 18,134,534 pounds; 1888, 35,635,714 pounds; 1896, 50,498,860 pounds; 1908, 25,938,500 pounds; 1919 to 1923, 13,236,948 pounds.

[13] The output of whitefish eggs and fry has been as follows: 1925, 172,970,000; 1924, 372,780,000; 1923, 537,546,000; 1922, 623,100,000; 1921, 420,450,000. Total, 2,126,846,000; average, 425,369,000. 1920, 390,365,000; 1919, 310,365,000; 1918, 484,032,000; 1917, 384,212,500; 1916, 391,155,000. Total, 1,960,129,000; average, 392,026,000. Ten-year total, 4,086,975,000; 10-year average, 408,698,000.

animals were almost equally devoted to taxonomy and morphology. While the fish-culturists produced relatively few publications during this period, their actions spoke louder than words, for the artificial propagation of nearly every animal of economic value, vertebrate and invertebrate, was undertaken; practical inventions of all manner of apparatus, from egg trays to fishways, were perfected; and extensive efforts at transplanting and acclimatization were made, with brilliant results in some cases.

Since 1900 the policy of the bureau has undergone a gradual change. Partly as a result of the maturing of this science, and partly because of the general trend in research in the universities throughout the country, investigators turned their interest from systematic ichthyology to the experimental branches, and papers on physiology, embryology and behavior, habits, or natural history of fishes appeared in increasing numbers. Indeed, papers on the taxonomy of fishes were reduced in number from 71 per cent to 28 per cent of those on biology of fishes (or from 13 per cent to 7 per cent of all documents published). Publications on fish propagation indicate an increasing interest in pond culture; and in the fisheries, less attention has been given to reconnaissance surveys and more to the economics and technology of the fishery industries.

This changing attitude is shown further by the interest displayed in the study of habits and behavior of fishes, which later has become expanded into studies of life history. As the publications in systematic ichthyology decreased in number, those on natural history of fishes increased, and even those papers dealing with fishery surveys have given more attention to the habits of the fishes considered.

It may be assumed, in the absence of evidence to the contrary, that this growing attention to studies on the life histories, with its many immediate practical bearings upon fisheries conservation, met with popular approval; for. beginning in 1908, the appropriations began to take a distinctly upward trend, although they had been virtually horizontal for several years previous. It was at this time that the brilliant researches of LeFevre and Curtis on the life history and artificial propagation of the fresh-water mussel were published; and marked progress in the understanding and control of oyster production, as initiated by the work of H. F. Moore, resulted in increased interest in practical problems of fishery development.

The increase in financial support continued steadily, with but a slight check, until in 1918 appropriations had increased 150 per cent over those in 1908. Beginning in 1918, however, a period of retrenchment, corresponding with post-war deflation, began, and resulted in an actual decline in total appropriations, reaching a low level in 1923.

It may be of interest to consider in a little more detail the support given to scientific investigations of the bureau, as indicated by congressional appropriations. Figure 15 is a historogram of the appropriations for scientific inquiry since 1894, in which salaries and miscellaneous expenses are indicated separately and together. It is very difficult, because of the method of accounting, to determine exactly the amount of money spent each year by this division on strictly scientific investigations, partly because of the fact that there has been extensive cooperation between the various divisions, partly because there has been an overlapping of activity, but particularly because the account of vessels for all purposes has been kept separate, and it is impossible to segregate that part spent on purely biological work. I have included the expenditures made on the steamer *Albatross* (not including extensive overhauling and repairs, and not including the pay of the Navy crew). The upper line, therefore, represents this additional amount and also an estimate of the cooperation afforded by the Alaska division, particularly on salmon investigations. The serious decline in total appropriations from 1920 to 1923 is thus apparent. It would appear that this curve of expenditures approaches some form of exponental curve, and that its rate of increase is, therefore, fairly uniform. This is more apparent if the curve before 1920 is considered. In order to show more clearly the rate of increase I have plotted these figures on a logarithm basis, as in Figure 16.

If my assumption is correct that the curve of expenditures is represented by the formula $y=ax^b$, the trend on the chart will be a straight line. Although there is some tendency to break about 1911, the curve apparently does fit a straight-line trend during the two decades from 1900 to 1920, and hence it may be concluded that the expansion of scientific investigations has progressed at

a fairly consistent rate. It is undoubtedly a common conviction, which may have been fostered deliberately in some quarters, that the investigations of the Bureau of Fisheries have suffered serious curtailment for lack of support. By comparing the rate of increase in appropriations for fishery research with that of all other civil expenditures of the Government, it will be seen that prior to the great war the fisheries received proportionately greater support from year to year than did other governmental activities; but if it be assumed that it is proper to spend a definite proportion of the Nation's wealth upon the conservation of the fishery resources, then it would appear that investigation of the fisheries has suffered to some extent for the curve of total wealth of the Nation rises somewhat more rapidly. Of course, the activities of the Bureau of Fisheries are but a part of the total effort expended in the whole country upon fishery conservation, but inasmuch as the field of endeavor is still so vast, it is not unreasonable to look for a parallel development of total wealth and of Federal conservation effort. As a tremendous setback occurred following the

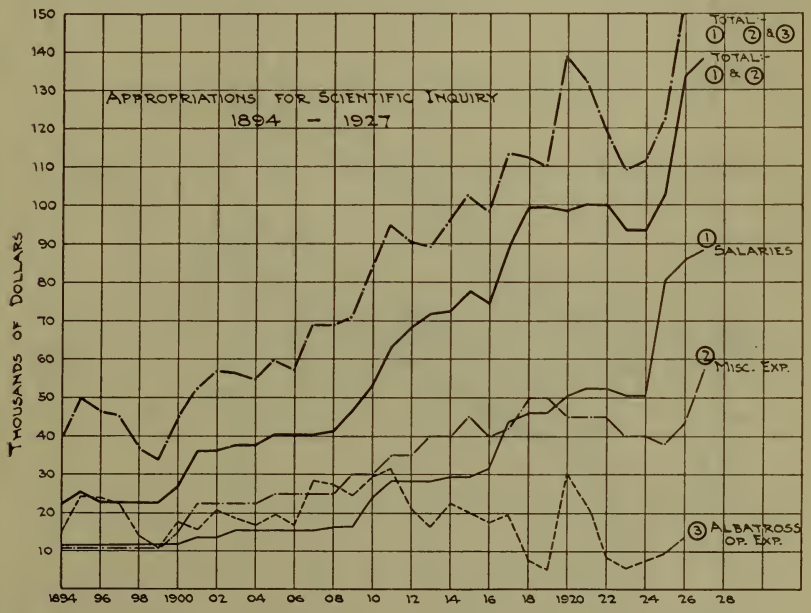

FIG. 15.—Expenditures for scientific inquiry of the bureaus, 1894 to 1926. Curve 1, appropriations for salaries of the scientific staff; 2, miscellaneous expenses; 3, operating expenses of the steamers *Albatross* and *Albatross II*, which are chargeable largely to scientific work. Total curves appear above and the upper one includes some additional expenditures made by the Alaska service in the interest of salmon research

war it is but natural to expect a proportionate increase in activity in the coming years, and if the trend is to be maintained the appropriations for scientific inquiry should reach approximately $275,000 by 1930.

In 1924, by an act of Congress of far-reaching importance, the salaries of the employees of the division of scientific inquiry, through the reclassification act, were increased approximately 60 per cent. By thus placing the standard of compensation at a level comparable to that in State universities, the employment of specialists adequately trained to produce worth-while contributions to the knowledge of fisheries conservation now is possible. Whether or not we are successful in carrying out this program of expansion depends upon the success of the scientific staff in producing results of the highest scientific standing and the utmost practical value. The promise of the future is rich, indeed.

Before reviewing the modern tendency in fishery investigations, particularly since 1920, consider the state of the fisheries of the country and the outstanding problems that they present. The total yield has continued to increase since the earliest survey in 1880, until the annual yield, including that of Alaska,

exceeds 2,900,000,000 pounds, valued at more than $90,000,000. While it thus appears that the fisheries, on the whole, have prospered, there is convincing evidence that many of our great fisheries are suffering actual depletion. I need not dwell upon this fact, for it has been treated fully already to-day.

The condition of the fisheries is even more alarming, however, when we consider the possibility of future development. While it is true that the Ameri-

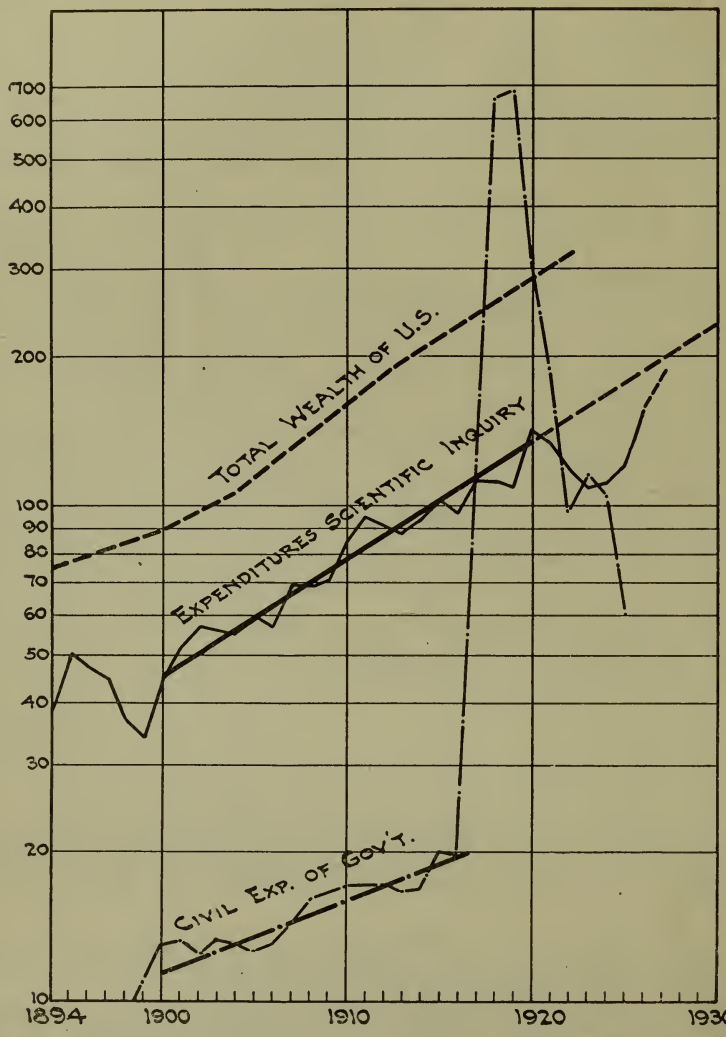

FIG. 16.—Expenditures for scientific inquiry of the bureau, compared with all civil expenditures of the Government and with national wealth (expressed as mantissas of the logarithms of dollars)

can Nation is not a fish-eating people, the great importance of sea food in the balanced dietary, recently demonstrated, makes the fisheries an indispensable source of healthful food. The increase in population of this country during the last decade exceeded that of any previous period and will, in all probability, continue to increase for many decades. Moreover, recent developments in refrigeration, preservation, and distribution of sea foods, together with improvements in marketing, compel the conviction that we are on the threshhold of

an era of exploitation of both fresh and salt water fishes, the extent and intensity of which, on the basis of past experience, is beyond our ability to estimate. The one great outstanding problem of the fisheries, therefore, overshadowing all others in fundamental importance, is that of maintaining an abundant supply. The time has passed when our fishing grounds may be extended and new reserves drawn upon sufficient to meet the demand. The only hope of the future lies in the wise husbanding of our present resources.

It is, therefore, clearly the duty of the Bureau of Fisheries, in the fifty-seventh year of its activity, to return to the faith of the fathers and, in the words of Spencer F. Baird, to ascertain what diminution in the number of food fishes of the coast and lakes of the United States *has* taken place, to determine what are the causes of the same, and to suggest any measures that might serve to remedy the evil. An official of the Bureau of Fisheries has made the profound discovery, the honor for which I am sure he will share gladly with hundreds of codiscoverers, that the *real* cause of the deterioration of our fisheries is the lack of adequate regulation! This discovery is valuable as the basis for an excellent alibi, for the fisheries are the property of the States and are under their sole jurisdiction, hence the responsibility for the decline rests upon the State governments, which have permitted excessive destruction and wasteful methods of fishing to continue unabated. It means more than that, however. It epitomizes the changed attitude toward the all-sufficiency of fish culture in maintaining the fisheries.

Of course, the announcement of this discovery does nothing more than define the responsibility, for the lack of adequate regulations is the result of inadequate knowledge concerning the proper methods of conserving the fish supply and public apathy, in some localities, toward making use of the knowledge already had. In its rôle as advisor to the States, it is the duty of the bureau to determine the variations in the supply of commercial fishes and the factors that regulate their abundance, and to encourage the application of this knowledge to their protection and wise use. While fishes have been studied heretofore as individuals, they must now be studied as species or as natural units—populations that inhabit limited areas. A fisheries science must be developed comparable to the science of vital statistics—an idea advanced by Dr. Johan Hjort before the International Council for the Study of the Sea in 1907.[14] As he pointed out, the three most prominent factors affecting populations are birth rate, age distribution, and migration, and these must receive our first attention.

While an intimate knowledge of the life history of the commercial species of fish must always be the basis of every attempt at conservation, the husbandry of fish must depend upon other information, chief of which is an accurate knowledge of changes in the abundance of the fish stock. The fishery itself must be studied therefore, together with the effects of fishing effort upon the stock, in order to gauge the increase or decrease in available supply and to discover if these changes are due to natural causes over which man can hope to have but little control, or to overfishing, and therefore subject to regulations. Effective fish husbandry must further take into account the proper regulation of the fishery, so that the yield may reach its maximum without encroaching upon that reserve of spawning stock upon which the future must depend. Another phase of fish husbandry, and which applies to inland waters, is concerned with fish farming. Methods of propagation, of cultivation, and of management must be devised to supplement and increase the productivity of natural waters and to make fruitful the barren areas wherever possible.

To summarize: 1. The bureau must assume part of the responsibility for maintaining and building up the commercial fisheries, and its investigations must be directed toward that end.

2. It must actively encourage the States to do their part in gathering statistics and cooperating in investigations.

3. The bureau's investigations of commercial fisheries must include observing and describing the composition of the catch and the fluctuations in supply, as indicated by reliable criteria of abundance. Natural fluctuations must be distinguished from depletion resulting from man's activities.

4. The life histories, migrations, and ecological relations of the principal species must be worked out to explain the fluctuations in the fish stock and, if possible, to control or foretell their occurrence.

[14] Rapports et Procés-verbaux, Vol. XX.

5. In those fisheries more directly under the control of man, such as river, smaller lake, and mollusk fisheries, the principles of aquiculture and the science of water farming must be developed.

6. These problems are so great, and the need of their early solution so urgent, that investigations into other phases of ichthyology, oceanography, limnology, physiology, zoogeography, and related branches of aquatic biology should not be undertaken independently, but only as they relate to a proper understanding of these immediate problems. This is not an attack on "pure science," but a plea for "purer" fishery science.

The rapid development of natural science in America fills one of the most brilliant pages in our history, yet our fisheries have declined, some of them have been destroyed perhaps beyond all hope of recovery, and all have failed to develop to their fullest state of usefulness. This willful disregard of the interest of posterity in the natural wealth entrusted to our keeping is a reproach upon the American system of democratic government and a challenge to American brains to devise the means for its protection. We must accept this challenge or merit the resentment and blame of the unborn future.

Mr. HIGGINS. It has been suggested that the remainder of the afternoon be devoted to free discussion of all of the problems that have been raised to-day, and I think it would be proper for us to discuss them as a group and then spend the rest of the afternoon in informal discussion in smaller groups.

Mr. RADCLIFFE. I think we should realize what a resource to this country the fisheries represent. A resource producing some 1,400,000 tons of products used for human food and products used in the arts and industries. Both Mr. Thompson and Mr. Higgins have emphasized the fact that we are at the beginning of an era of exploitation. I should like to give you some indication as to how far that is going on outside of our own borders, as well as within. With the falling off of the catch of the North Sea, immediately the European trawlers began to fish around Iceland. Now that those waters are showing the strain they have turned to Greenland. In 1924, the Norwegian Government sent a vessel to Davis Strait to determine something of the fisheries resources off Greenland. Last year between 30 and 40 European vessels went up there for the purpose of exploiting the fisheries. The present year, I understand, that activity has been continued.

The whole trend is to build larger vessels—vessels better equipped for exploiting fisheries of the whole globe. In our own country we have seen the advantages in the improved methods of merchandising fish, such as the development of filleting and brine-freezing—matters of that kind which are building up the industry.

One of the most alarming situations, in so far as our fisheries are concerned, has to do with the whale fishing. Heretofore it might have been possible to have exercised some measure of regulation over these fisheries. At the present time they are building vessels capable of operating on the high seas, taking the whale aboard the ship, manufacturing it into products that have a market value, without ever touching at any port, and then steaming to the port offering them the best prices for their products.

Mr. Scofield told you something of what is happening off the coast of California, where the concerns are anchoring outside the 3-mile limit. A vessel has been constructed recently in France, allegedly for the purpose of operating off the coast of Africa. This vessel has a refrigeration plant and storage for seven or eight hundred tons of frozen fish; eight retorts, part of which are for cooking lobsters and

crawfish, some for the reduction of fish into oil and scrap; a storage tank that will hold 26,000 gallons of oil; another tank for holding 400 gallons of oil; and the statement has been made that if the African venture is not successful, they will turn to Greenland or even to our own North Atlantic banks.

The problems of the fisheries are no longer State or National, but international. How are you going to govern these things unless it is through international agreement? The trend and condition of a fishery can be ascertained only after long study and research. Take our own North Atlantic fishing banks; we do not know what the conditions are that obtain there. Next year we may have a "rum row" exploiting these fisheries in a manner unparalleled in our history, and the only way we can control these conditions is by a "round table" of international statesmen and diplomats to solve such international problems. The biologist must produce something that will show us where these fisheries are bound, and then pass it on in a form to be sold to the diplomats and statesmen for their use in order to get proper regulation.

I have the deepest sympathy for the fisherman. I think that too often we forget the conditions under which the individual fisherman operates. Our fishermen receive about $97,000,000 for their harvest. Less than $500 per fisherman. Think of them in the winter; two of them in a small dory, operating in the lanes of steamers, facing death, under all kinds of weather conditions. Nearly every week the papers in New England tell of some fishing vessel returning with at least one or two of the men missing from the crew. The men who operate under these conditions can not be expected to have a perspective that will enable them to say just what is best for the fishery as a whole.

Mr. Scofield. I would like to hear some one discuss a question I would like to bring up. I had more or less to say about the use of the sardine in California, outside of the 3-mile limit, for reduction purposes. I have been asked the question, "Why do we care if they use the sardine for reduction purposes, if our investigations have not shown that the sardine is decreasing in abundance in California?" The sardine is being guarded against exploitation by the reduction plants within the State, and I was going to suggest that as a topic. I think there are some here who might have something to say along that line.

Mr. Higgins. I think Mr. Thompson has thought on that question perhaps as much as any of us. I am going to ask his opinion.

Mr. Thompson. Mr. Sette showed on one of his graphs the enormous importance of the menhaden fisheries. That importance is entirely due to the expansion in the market for oils. I can see where the possible limit to expansion is—that is, a fishery that will take absolutely everything that the net brings in—and some time in the future we may find ourselves with a situation we can't very well control. I think Mr. Scofield's attitude in advocating the restriction of such fisheries as the sardine for fertilizer is a sound one as a general state policy. The danger is not so much what may happen to-day as it is allowing the growth of a great industry that can not be controlled when the time comes to control it.

Doctor Rich. I have always wondered why edible fats might not be as legitimate a use for a fishery as food alone. Might not the

by-products serve just as great an amount of good as the actual food? Should all the fisheries, then, be reserved solely for the use of food, if they can be so reserved?

Mr. Thompson. Edible fats would reach the people in the form of fish; but to take one-tenth of the value of a fish, when you might possibly build up the use of the whole fish, I think would be a mistake.

Mr. Scofield. We have one factor in California that is operating under a modification of the law which certain persons were able to bring about. They are making fish flour. The process is not very different from an ordinary reduction that is carried on in an ordinary reduction plant. The fish are run through by machinery. Then the product is ground and run through the mill, and the final product is fish flour. It is not much more expensive to produce than ordinary fish meal. About three-fifths or two-fifths of the product is bone and gross material which goes as fertilizer, anyway.

The law was also modified to permit the use of sardines for the oil, provided the oil be used for edible purposes. The company extracting the oil must refine it for use as human food. Two rather large concerns made use of that modification last year.

It seems to me that if the companies are permitted to take the sardine for the oil and use the oil as a food there will be no limit to their activities. It would be the same thing as permitting a reduction industry to take fish for fish meal or fertilizer; so we hope to have striken from the law the special privilege of taking fish to make oil.

If the supply is exhaustible (and we are not sure that it is not), I think our scheme should be to guard against the establishment of a reduction industry, and should be in favor of the use of the sardine or herring as human food. The use of the sardine, at least for canning, gives employment to a great many more people than if it were used solely for reduction purposes. As Mr. Thompson pointed out, the great danger is in permitting a great industry to be established which we will be unable later to curtail or to put out of business when we find that the resource that is being drawn up is not sufficient.

Mr. Higgins. Captain Wallace, what is your idea on this subject with reference to menhaden?

Captain Wallace. The menhaden is not a food fish. It is used only for making fertilizer. There is this viewpoint in the manufacture of a fish like the menhaden into oil and fertilizer: There is being given to the country something that otherwise would not be utilized. The fertilizer that is being used on the farm is also of economic good to the country in rendering the land more productive.

Doctor Galtsoff. I would like to ask a few questions about your paper, Mr. Higgins. It seems to me one important thing was missed entirely. I understand perfectly the fundamental ideas which show the necessity of statistical work, but it seems to me the statistics are sound only if we have a good biological understanding of these particular species. That is true for every type of biological work. Mathematics is all right, but there must be some biological background to which to apply the mathematical formulas. In a general way, the understanding of the biological background requires a perfect understanding of the organism—the understanding of its environment and

its habits. I heard nothing about the study of its environment, which is absolutely necessary for understanding biological factors. One thing particularly refers to the environment in which our fish and other marine organisms live. That is the question of pollution.

Everybody who has had experience in the work of the Bureau of Fisheries knows that sooner or later, directly or indirectly, this question certainly is brought to his attention. The question of pollution has two aspects. One is sanitary, which I regard as outside the scope of the bureau. That is up to the health department, to determine whether the oyster we are eating is safe. The pollution kills the marine organisms or makes them unpalatable. In this way, certainly, it is a primary question for the Bureau of Fisheries to decide. The question of pollution is certainly complex. It is not a State affair. It very soon will be an international question. So far it is not, but maybe in 20 or 25 years, with the development of sea communication, it will be international. It seems to me the study of pollution, or the effect of pollution on marine organisms, certainly must be put on the program of the scientific investigation of the Bureau of Fisheries. If I am allowed to make this remark at present, I think we have no special laboratory, or agency, or organization to go into this situation, and so really the question is neglected.

We have found from our experience in the oyster division, say, that probably every third or fourth oyster comes directly in contact with pollution. Then there is a question of oysters along the Connecticut shores—the question of why the oysters become unpalatable, and so on—because of domestic sewage pollution, trade wastage, and oil pollution.

It probably requires the joint efforts of both the economist and the biologist. I wish only to call your attention to these facts, which probably are not known in a general way. I looked into the records of the New York Metropolitan Sewage Commission and found that New York discharges about 1,000,000,000 gallons of sewage daily into the water. The theory that this is carried out to the ocean is wrong. It just washes back and forth within 15 or 20 miles and stays right there. Notwithstanding the activity of the Public Health Service to secure strict enforcement of strict regulations with reference to oysters, we have the following situation: Last year we went to the localities that looked to be safe from any pollution. The request was to show to Georgia State officials the grounds suitable for oyster cultivation. The Public Health Service made a survey at the same time. I found, to my great disappointment, that all the best localities, the most suitable for the cultivation of the oyster, were condemned by the Public Health Service. It would take too long to discuss the reasons for condemnation, but I didn't think they were reasons to condemn, because I questioned the methods of this survey, which was made during the tremendous flood. But still there are certain areas in the South, far from obvious pollution, which can possibly be polluted and are not suitable for the production of oysters.

I saw, in the Gulf of Mexico, thousands of acres of grounds bearing good oysters that are entirely unpalatable. They are polluted by oil and acquire a little oily taste, which makes them absolutely

unpalatable. Fish there also acquire this oily taste. So really this question becomes more and more serious. It may set aside entirely all the statistical investigations that are made on a certain section. There will be no scarcity or decrease in abundance of fish: simply, they will migrate in a different direction. They don't go into these polluted or semipolluted waters. This must be taken into consideration.

I think it will be absolutely necessary for the division of scientific inquiry to put this question of pollution on its program, from the point of view of conservation.

Mr. Radcliffe. It may be well to review very briefly the story of the bureau's attitude toward this question. I am in sympathy with Doctor Galtsoff's attitude. Some years ago the bureau sought an appropriation and failed to get it. About two years ago, after hearings that lasted several months, it was very evident that Congress was chary about taking away the rights of the States to handle these particular problems. There were, also two schools of thought—one favoring the War Department and the other the Department of Commerce for the study of these problems. The War Department won. When the act was passed finally, the War Department was given authority to put the law into effect, and we have been a little timid about stepping in. Sooner or later, however, we will have to enter that field.

Mr. Higgins. Doctor Koelz, do you see any signs of an international problem of pollution?

Doctor Koelz. A little in Canada. The fishermen that were arraigned on the charge of catching small fish in Lake Erie pleaded that the Detroit River was killing them off anyway. That is about the only situation, because the other lakes are too wide to have a general bearing.

Mr. Higgins. It is your opinion that pollution is localized on the American side of the Great Lakes and is not present on the Canadian side?

Doctor Koelz. The Canadian side has no cities at all. The pollution is relatively nothing on the Canadian side.

Mr. Sette. Doctor Galtsoff has brought up the question of what good are statistics if we haven't the biological background, and I agree with him. However, I question the value of a biological background without statistics. Even pollution may enter the question. Our yield of anadromous fishes declined earlier and more rapidly than our other fishes, not saying anything about the abundance. We have taken fewer anadromous fish in recent years than we have in the past. That may very well be because they are subject to river conditions, which, in turn, are more influenced by pollution than the sea. The point I wish to make is that we couldn't even know we had a problem in pollution if we didn't know that the yield had declined in the rivers; and if you are going to attack any problem in fisheries you will have to know what the yield is, and, further, you will have to have a measuring stick of abundance based on the yield.

Inasmuch as our problems are on the conservation of the fisheries, we are faced continually with the questions of the yield, fluctuation in the yield, etc., which can not be taken up unless we know that

there are fluctuations and what these fluctuations are; and so, even when deciding which species you are going to study, you must have statistics of the yield, and when you get into the study of them you must have continuous reference to statistics.

Being more or less responsible for the conduct of the statistical department of the bureau, I realize very fully that our statistics are woefully inadequate. In the first place, we have statistics only once in a while on any particular region—once in half a dozen years. We may hit the peak or trough of any fluctuation of any fishery, so that the picture is very rough, indeed, if not entirely valueless, for analysis of an investigational nature. If we could have annual statistics we would be so much better off, because we know at least where our peaks and troughs are, and the biologist investigating the fisheries would learn why those peaks and troughs occur.

It has been more or less definitely expressed as the policy of the Bureau of Fisheries that we should develop the matter of collecting statistics by the States. That discussion, I believe, has appeared in the last two commissioners' reports and also in the last two reports of the division of fishery industries, and I should like to ask the biologists of the Bureau of Fisheries to consider carefully what it means to have this policy go through. You will be driven constantly to the use of the facts brought out by the statistics, and unless you have them you are at a loss. They are just as necessary as to find the ages and rates of growth of your fish or the ecological factors. As you go to various parts of the country you have opportunity to come in contact with men in various industries, with State officials, and with various persons interested in fisheries; and I think it would help a great deal in advancing this feature if each one of you would be more or less of a missionary in educating people to an understanding of this vital necessity in our fisheries. Educate the public so that they will appreciate the necessity for statistics and will actually have enough interest to support a movement in this direction.

We, in this division, would always welcome suggestions from the biologists as to how we can best present statistical material. I don't know whether you noticed or not, but in the last report of this division, instead of showing the statistics of each county in the States for which we published statistics, we confined our presentation to State figures only. We did this for two reasons—for economy in publication and clarity in presentation. We discussed these particular changes before they were made. No one could decide whether or not it should be done, and we finally decided that the only way was to do it and see what objections would be raised. I just wish that everybody who has occasion to use any of these statistics would offer suggestions for improvement of presentation or improvement of collection, or for any additional statistics that it may be feasible to present in our annual publication.

REVIEW OF FISHERY INVESTIGATIONS CONDUCTED BY THE BUREAU OF FISHERIES

NORTH ATLANTIC FISHERIES

GENERAL REVIEW

By Dr. H. B. Bigelow

I didn't come here with any set speech, so shall say only a word of reminiscence about the North Atlantic investigations.

The life histories of the fish that support the great North Atlantic fisheries have been slighted in American waters—cod, haddock, pollock, mackerel, herring—and when it became possible to undertake such studies a few years ago the two problems that seemed to promise the most fertile field were the lives of the cod and mackerel.

The cod has been the backbone of the North Atlantic fisheries on this side of the Atlantic, and a good deal of attention has been paid to the life history of the cod on the other side of the Atlantic, so the basic facts of the cod's life history are well understood. Nothing, however, was known about the migrations of the codfish in American waters nor about what conditions favored years of high production (and so good fishing) or what hindered years of high production; and until some light was thrown on these questions it was obvious that the bureau never could outline any rational means of conservation if the fishery should show signs of depletion or tell the fishing interests how they could increase their catches if there was opportunity and if the fish market would take them.

The simplest problem to attack in the case of the cod was migration; and so, in 1923, the bureau started tagging codfish. Fortunately, the codfish proved to be a very favorable fish to tag. We have some codfish that have been tagged and recaptured twice, on which the tags look perfectly all right—the cod has been perfectly fit and happy and his rate of growth hasn't been set back. Consequently we have every reason to think that the cod returns are significant, even though many of the fish lose their tags.

Tagging has been carried on from 1923 up to date. During the first years the bureau was limited in the field that could be covered by the lack of a seaworthy ship. Those of you who have worked at sea know that on the high seas, often in winter, you must have a ship that is seaworthy if you want to do the job. Now the bureau has become possessed of a ship that is seaworthy; she can go anywhere; so this last year the bureau was able to spread its tagging operations to Georges Bank, and in the future there is no reason why such work can not be carried out anywhere on the offshore banks.

While the cod tagging has been going on, haddock and pollock have been tagged too, and data have gradually accumulated to show the migrations of these species as well. Mr. Schroeder has worked up the report on the migrations of the codfish which he will present to you shortly. As to the factors that control production, etc., a start was made two winters ago by the *Fish Hawk* in Massachusetts Bay. Very satisfying things came out as to the involuntary drifts of the eggs and larvæ. We knew previously from European experience that cod eggs and larvæ, like any other buoyant eggs, drift where the water takes them. And it does not follow because a lot of larvæ are found in one place that they were spawned there. Doctor Fish will tell you about this one actual start that has been made in the Massachusetts Bay region on that problem. Mr. Sette can tell you about the mackerel.

I might add that all the work carried on at sea has been accompanied constantly by an oceanographic program, yielding records of the temperature of the water, of the salinity of the water, and of the nature and abundance of the plankton, data which, when tied up with the production of eggs, may tell us what factors favor high years of production and what things go against it. We begin to suspect that it is not so much the physical character of the water. In fact, Doctor Leim's experiments on cod eggs show that the cod egg is almost foolproof. You can do almost anything to it and still it will hatch successfully. The great loss apparently comes after that, and we begin to suspect that it is the predatory members of the plankton that are chiefly to blame, combined with the scarcity of food for the larval fishes.

I think, perhaps, that this is sufficient introduction to the lines of attack. I might say that we also begin to see ahead the possibility of predicting whether, in any year, the stock of fish is going to be big or small—in the case of the mackerel, for instance, whether it is going to be worth while to go mackerel fishing or not. For years the mackerel fishery has been subject to tremendous fluctuations, the catch varying from practically none to hundreds of millions of pounds. Until these investigations started there was no way of envisaging at all whether these fluctuations reflected movements of the fish or whether they reflected actual fluctuations in the number of mackerel existing in the sea at the time. We are now in position to say that they do reflect actual fluctuations in the numbers of mackerel.

COD STUDIES

By W. C. Schroeder

My problem is concerned with the life histories of the cod, pollock, and haddock. The three species are found under much the same conditions, so that their combined study is practicable; but special attention has been given to the cod, and the data for this species are more complete than for the others. These are important food fishes, found on both sides of the North Atlantic, and are worth over $5,000,000 annually to our fisherman.

Migrations, rate of growth, and age with respect to size are the chief factors under consideration. Doctor and Mrs. Fish have been studying the drift of the eggs and larvæ, while I am interested chiefly in fish upward of 1¼ inches in length. It is at about this size that the cod takes to the bottom and the first scales appear.

We know very little about cod less than 10 inches in length or less than 2 years of age, and this is one of the gaps we are endeavoring to fill. Fish of this size appear to be scarce along our immediate shores, for repeated seining around Woods Hole and along the entire New England coast has produced but very few specimens. Offshore we have done a limited amount of dragging with beam trawls, otter trawls, and circular nets; but on smooth, sandy bottom we catch next to nothing, and on rocky bottom the fishing gear is nearly always damaged or lost. Last summer we were fortunate in making several good hauls on northeast and southwest Georges Bank, where the bottom was somewhat rocky, yet not enough so to destroy the gear. Among miscellaneous fishes, about a dozen cod and many haddock, 2 to 5 inches long, were caught. It seems logical to conclude, even on this meager evidence, that the habitat of very young cod includes all our offshore cod grounds. In changing from the pelagic to the demersal stage, those fish that happen to go down on or near rocky bottom ought to have a much better chance of survival than those that chance upon mud or sand, where they afford an easy prey for carnivorous fishes.

The smallest caught along our shores are usually more than 10 inches in length. The shore waters of Maine form a refuge for cod of 10 to 20 inches, and fish of these lengths are so abundant there that some sort of migration from offshore evidently takes place.

In an attempt to throw light on migrations, 41,000 fish were tagged from 1923 to 1926, of which 28,000 were cod, 5,000 were pollock, and 8,000 were haddock. Operations were carried out by the *Halcyon* up to 1925 and by the *Albatross II* during 1926. The fishing grounds comprised many localities from eastern Maine to southern Massachusetts, but most of the fishing has been centered off Mount Desert and on Nantucket Shoals.

All these fish were caught with hand lines and by the same method of fishing as is employed by commercial vessels. Generally we fished in water less than 25 fathoms in depth, for beyond this depth an increasing number of fish are unsuitable for tagging, owing to their sudden removal from the great pressure which exists on the bottom. In shallow water we lose from 5 to 10 per cent of our fish, due to injuries received when they are hooked, and in water as deep as 40 to 50 fathoms the losses due to pressure and hooking combined amount to from 15 to 20 per cent.

After a fish has been landed it is laid on a measuring board and a tag is clamped to the upper part of its tail near the base. A few scales are scraped from along the side below the first or second dorsal fin and the fish is then returned to the water. These operations require about 20 seconds per fish.

The scales and measurements are placed in books numbered the same as the tags.

A metal tag is used. Various metals were tried, none of them entirely unsatisfactory, but monel metal was adopted finally as being noncorroding and comparatively cheap in price.

One of the chief difficulties in using tags of any description is the effects of an irritation set up around the point of attachment. After several months, or more, many of the tags will pull through the softened tissue, out along the membrane of the tail, and be lost. It is estimated that 60 per cent of the tags are lost in this manner within the first year. The irritation of the tag does not appear to affect the physical condition of the fish except at the point of attachment, and in some cases the wound heals nicely.

About 1,700 tagged cod have been reported recaptured, of which 350 were taken by our vessels *Halcyon* and *Albatross*, most of them a month to a year after the fish were tagged. These latter records are of particular value, for with them it is possible to determine the rate of growth with reasonable accuracy and to note how this increased growth is registered on the scales. Locality records, the condition of the fish, and other data are more precise when we make our own recaptures.

The percentage of tag returns depends largely on the intensity of fishing and the locality of tagging, whether close to land, on a cod ground of restricted area, or offshore, where the grounds may be a thousand times as large. For example, as many as 20 to 35 per cent of the cod tagged on certain cruises have been recaptured off Mount Desert Island, Me.; whereas we have not yet received a single return from 1,000 cod tagged last August on Georges Bank, 150 miles from our shores.

The results of the tagging have several points of particular interest. According to tag returns it was found that cod east of Cape Ann, Mass., do not migrate south toward Cape Cod, except as stragglers; but quite a few Maine fish migrate east to Canadian waters. Of about 10,000 cod tagged east of Cape Ann, only 1 has been reported recaptured south of Cape Cod, and we caught that fish ourselves; and of nearly 18,000 tagged on Nantucket Shoals, we have only one reliable recapture record from as far east as Maine. Between Maine and southern Massachusetts we have tagged a few cod in Massachusetts Bay, and recaptures of these fish have been made both to the eastward and to the southward.

The most impressive cod migration that we have found occurs each fall from Nantucket Shoals to Rhode Island, Long Island, New Jersey, and Delaware. Our most southerly recapture record was off Cape Henlopen, Del., but we know that cod migrate as far south as North Carolina. Within the region west and south of Rhode Island virtually no cod are present in summer. In the fall the first cod appear off western Long Island and off New Jersey the last week of October. The first recaptures of our tagged fish have occurred each year in this region between October 27 and November 4, and the last during April. In Rhode Island waters cod are rather abundant from fall to spring, and a few fish remain over the summer, but not enough to support commercial fishing.

Speaking of all localities along the New England coast where we have tagged it was found that cod may remain for long periods in one immediate locality, sometimes for as long as 1 or 1½ years. We have tagged fish very close to buoys and lighthouses or other convenient marks and returned one year later to recapture the fish in exactly the same locality. Not only has this happened to individual fish, but small pods of cod have shown a tendency to remain intact for a considerable time. We have more than one record where nearly consecutive numbers were taken a year later in the same locality on the same day; and we have several records of consecutive numbers taken within a few minutes of each other several months after the tagging date.

Scale samples have been taken from all fish tagged since 1924, inclusive. When a fish was recaptured by our vessels it was again scaled, of course, and measured, The data obtained from these recaptures are of great importance for it is our only positive method of learning how growth is registered on the scales. The scales from these recaptured fish prove that an annulus is formed. What is usually called a zone of winter growth on the cod scale might better be termed "zone of retarded growth," for I have noted that the so-called "winter rings" begin to form as early as September and October.

Over 10,000 scales have been mounted, and of these about 10 per cent have been studied. Scale study has not been pushed vigorously because of an uncertainty existing as to the early growth. The long spawning period of the cod

(from October to April) makes it particularly difficult to interpret the first year's growth. To assist in this all young cod, as well as pollock and haddock, are preserved.

According to scale growth, I find that cod from southern Massachusetts, of 1. 2, and 3 years of age, are about 7, 14, and 21 inches in length; and cod from eastern Maine are 6, 12, and 18 inches in length. After the third year, the increase in length becomes less with each added year. Our recapture records show that the actual increase in growth made during one year by tagged cod from Maine was about 4 inches from the third to the fourth year, the same from the fourth to the fifth, 3½ inches from the fifth to the sixth, and 2¾ inches from the sixth to the seventh year. Year classes, as determined by length frequencies, compare well with the lengths of the fish as calculated from the scales, at least up until the fourth year. Little can be done with the length frequencies of the older fish.

The cod problem is a complex one and it is necessary to study the fish in many localities and under varying conditions. In general, large fish are found offshore and small fish inshore. The smallest cod of 1,000 caught on Georges Bank last summer was 20 inches long; whereas, out of the same number of fish caught within a few miles of shore, it frequently happens that as many as 80 or 90 per cent are below 20 inches. Each cod ground affording differing physical conditions has its own peculiar stock of fish, and there appears to be no general intermingling of fish from the various banks. It is true that large numbers of spawning cod collect in the shore waters of New England throughout the winter, but I am not prepared to say at this time where they come from or where they go after spawning. A few of our tagged fish have been caught among these spawners, but only scattering records and not enough to indicate a migration from the tagging ground.

Cod can adjust themselves to diverse living conditions; or at least we find them *living* under differing conditions. Comparing eastern Maine with Nantucket Shoals, both of which places have a year-round cod population, the Maine fish seldom have a bottom-water temperature greater than 11° C., which has been considered the upper limit for the cod. Yet Nantucket cod withstand as much as 14½° C. Maine cod feed largely on small crustaceans, such as shrimp and small hermit crabs, whereas Nantucket fish usually are gorged with large Cancer crabs. Maine cod appear to grow more slowly than Nantucket cod. The only migration shown by Maine cod has been entirely to the eastward to the coasts of Nova Scotia. Nantucket cod, if they migrate, go west and south, or to the Chatham grounds a few miles to the northeast. Out of thousands of cod tagged in eastern Maine, from 65 to 75 per cent were less than 18 inches in length; while on Nantucket Shoals, up to 1925, the percentage of these small fish was one-fourth to one-half of 1 per cent.

Another problem arises from the fact that while cod of one locality may behave the same for a period of years, an abrupt change may then occur. Nantucket cod, as regards the stock of fish and their migrations, were much the same for the three years 1923 to 1925; but in 1926 we found the stability of the fish population had in some way become unbalanced. Whereas, fish from 22 to 32 inches in length largely predominated during the first three years, in 1926 we found this group in the minority and that a new stock of fish, of 14 to 20 inches, had taken their place. Where the larger fish went and from whence the smaller ones came we have not yet ascertained, but are waiting for our tag returns to give some indication of what happened.

The cod program must continue for some years before we can hope to give a comprehensive account of the fish's life history.

Doctor RICH. What is the age of the small fish that came in this year?

Mr. SCHROEDER. There was a combination of two or three year fish. The 2-year-olds were extremely scarce the previous three years, but in the fall of 1925 we noted a change. Some of these small fish came in the fall of 1925. The previous fall some indication was noted that the change was about to occur when the small fish comprised about 8 per cent of the total, instead of one-fourth or one-half of 1 per cent, and then we thought something would happen. If you fish on any offshore grounds you can get absolutely nothing out of length frequencies; it seems as though all the lengths are represented,

so that we fish inshore and get enough of the small fish of 2 or 3 or possibly 4 year old size. We can not seem to get any of these sizes offshore.

Mr. THOMPSON. Did you analyze only the catches that you made yourself?

Mr. SCHROEDER. Yes.

Mr. THOMPSON. I then ask would the size of the hook make any difference in the class of fish caught?

Mr. SCHROEDER. It is true we did change the size of our hooks, but even then if young fish were present in any numbers at all you would expect to catch a few and the hooks are not so large that it was impossible to hook them at all. I have tried with one or two sizes; I have tried with smaller hooks and attempted to catch young fish, but have never succeeded.

Mr. THOMPSON. I am not familiar with the codfish or their fishing methods, but we might experiment with the dominant year classes. From my own experience with other fish, it is necessary to carry on a far more extensive analysis of catches—perhaps catches that were turned in by the fleet. You will find out, no doubt, that there are distinctive sizes in various parts of banks, so that before you could even attempt to demonstrate a dominant year class you would have to examine the commercial catch from all parts of the bank.

Mr. SCHROEDER. That would be preferable, but it was very hard to get all the fish that the commercial fishermen caught; usually this smaller size is almost worthless and the net return is so small they frequently throw them away so that it would be very hard to get any of the unselected catch. Furthermore, we usually fish 4, 5, 6, 7, or 8 hours, drifting all over, so that we have ample opportunity to cover a bank rather thoroughly. You may fish for an hour and catch nothing; then occasionally you catch fish 30 and 31 inches long; then you get no more of these big fish for 20 or 30 minutes; then another haul. We found, on Nantucket Shoals, that we would occasionally strike a body of other fish, and these would be smaller fish.

Mr. RADCLIFFE. Isn't it true that in recent years there has been a considerable increase in the intensity of fishing around that area—around Nantucket Shoals—and in the number of tags that have been taken from that area?

Mr. SCHROEDER. It has affected the cod more than the haddock.

Mr. RADCLIFFE. Is it possible that the increase in intensity of fishing has reduced the proportion of larger fish?

Mr. SCHROEDER. I really don't think so, because most of the fishing there is along the edge of the bank. Sometimes we do not see a commercial fisherman for two or three days around the grounds. Of course, they may be around when we are not there, but Nantucket Shoals has not been considered a particularly important cod ground in the last few years. Some years ago, in 1914, Nantucket Shoals and the Chatham grounds actually contributed more cod than all the other cod grounds along our coast.

Doctor BIGELOW. Speaking of the migration of the fish—you state that the fish migrated but were confined largely to individual banks. I do not get the connection between these two statements.

Mr. SCHROEDER. It is hard to see why the fish migrate, for we know some do and some do not. For example, if we tag 1,000 fish in eastern

Maine, perhaps 700 of these fish will stay at home until they are caught—they will be recaptured a week or a month, a year or two years later—but some of those fish, for reasons unknown, will start to migrate, and it is impossible to say why. The same thing happens to our Nantucket fish; some will migrate and others will stay on Nantucket Shoals. At first we thought the varied temperature was an important factor, and the coming of fall and winter would cause the fish to migrate to warmer waters; but we are mistaken in that, because the maximum temperature is not reached until late fall, and that is about the time the fish start to migrate west and south—when the weather is actually warmest. Unless they have an instinct which sends them on these migrations, I can not find any reason why they should migrate west and south.

Doctor GILBERT. Is the number of straggling fish great enough to indicate migration?

Mr. SCHROEDER. I really think it is. I think the reason the straggling fish are often recaptured is because the fishermen go to those banks were many of them migrate and catch the fish. Perhaps some go to places where no one fishes—places seldom visited by anyone.

Doctor GILBERT. About this different rate of growth in the southern and northern fish; did I understand you to attribute that to food rather than any racial habits?

Mr. SCHROEDER. It may be to food and temperature combined. I do not think it is racial habit. I really can not answer that question at this time, although I hope to at some later time. Unmistakably, southern fish do grow faster than northern fish.

PLANKTON INVESTIGATIONS

By Dr. CHARLES J. FISH

The plankton investigations that form a part of the bureau's North Atlantic program have two basic objects:

1. To determine the relative value of the various cod "banks" as production centers, and to determine whether local production is sufficient to maintain the inshore stock along the New England coast or whether it is dependent for its supply upon immigration from the offshore spawning grounds.

2. To investigate the factors that cause success or failure in the annual production of young fish (high or low breeding years), as indicated by the variations in the catches of the various "age groups."

The results of extended investigations by Norwegians have shown the existence of an intimate relation between the fluctuations in the numerical value of the fish stock and the yield of the great fisheries. This makes it possible to determine the fluctuations in the renewal of the stock from year to year by determining the numerical value of the various age groups in the catches.

My problem is concerned with what causes these fluctuations from year to year in the supply of fish and where are the sources of supply. Obviously the fate of any group that has pelagic young is solely dependent upon the success or failure of the young to maintain themselves in the plankton. There must be ample food available, and the winds and tides must remain favorable at least until they reach the stage when benthonic life begins.

In European waters it has been shown that the actual quantity of eggs produced is often not in itself a factor sufficient to determine the numerical value of a year class. A rich spawning year may produce a year class poor in numbers, while a large year class may have its origin in a year when the spawning was at its lowest. This has been observed repeatedly in the water about Lofoten. However, it has also been determined that we must look to the early

stages to find the conditions that determine the number of individuals in any year class. In discussing the cod, Hjort stated as early as 1914 that the numerical value of a year class apparently is determined at a very early stage and continues in approximately the same relation to that of the other year classes throughout the lifetime of the individual, and that such data as are available indicate that the most important determining stages are "the very earliest larvæ and young fry stages."

Therefore, if the numerical value of a year class is determined in the early stages, and the actual quantity of eggs produced is not necessarily the determining factor, then we must look to the pelagic eggs and fry for an explanation.

The cod-spawning areas along the New England coast fall into two natural groups—the inshore grounds and the offshore grounds. Because of the lack of a suitable ship, the former were selected for investigation first, with the field work centered in and about Massachusetts Bay. As a result of the investigations by Doctor Bigelow, there was available considerable information on the seasons of spawning, the seasonal variation in the plankton communities, and the general movements of the water in the Gulf of Maine. Drift-bottle experiments and observations on plankton distribution had established the existence of a definite southerly drift or set along the whole western margin of the gulf. This has long been known to the fishermen as the "so'west current." Fish eggs in progressively later stages of development have also been found as one approaches Cape Ann from the east. Again, the catches extending over a long period of years have shown that the bay itself harbors one of the largest inshore breeding centers—the Plymouth grounds.

The immediate problem involved a determination of the value of Massachusetts Bay as a production center and also as a nursery for the large numbers of eggs and larvæ that we had reason to believe were constantly being transported in as contributions from the spawning grounds to the east. Here the young fry could find ample food and be protected from the storms and ocean waves until large enough to care for themselves. It has even been suggested that this limited area might form one of the most important sources of supply for codfish on the whole Atlantic coast.

The work was to consist of an investigation of the Massachusetts and Ipswich Bay grounds, the conditions existing during the period of incubation of the eggs, the early life history, the food during this period, the enemies, and the distribution and migrations during the first year.

In November, 1924, 2 visits were made to the Plymouth grounds, and between December 3, 1924, and June 17, 1925, 14 cruises were made on the *Fish Hawk*, covering the area between Ipswich Bay and Provincetown. At 41 stations physical observations and net collections were made.

Charts based on the distribution of eggs in various stages of development show clearly the spawning center and the drift of the eggs, the greater proportion of late stages being taken about the Provincetown region. Drift bottles also substantiated the current movement as indicated by the eggs.

The results so far indicate that a very definite and constant counterclockwise drift carries all cod eggs spawned in Massachusetts Bay out before they hatch. Throughout the breeding season eggs were found in abundance, particularly about the Plymouth grounds, but the collections of the 14 cruises failed to yield a single young cod. Drift bottles took the same general course as the eggs, those not fetching up on the inner arm of the bay dividing after passing beyond Provincetown. A part of the latter completed the circle and appeared on the Nova Scotia coast, or, if they turned south, passing into the region about Nantucket. One apparently drifted farther to the eastward and was carried in the Gulf Stream to the beach of Lands End, Cornwall, England, where it was recovered on June 15, 1926.

Investigations carried on in Ipswich Bay, an important adjacent spawning ground to the north, yielded similar results, showing that here the eggs were carried out east or south, beyond Cape Ann, at even earlier stages than those of Massachusetts Bay.

We must therefore conclude that local production in Massachusetts Bay is not sufficient to maintain the inshore stock, and were it not for constant immigration from the outer waters the supply would soon be exhausted.

At the time, the lack of a suitable ship prevented an extension of the survey to the outer waters to determine the fate of the cod eggs after leaving the bay, the next logical step in the program. No doubt many of the eggs carried beyond the cape survive and perhaps help stock the outer banks, or else work

back when the bottom stage is reached, or both. Perhaps the young bottom stages entering the shore waters south of Cape Cod with large schools of small pollock, each spring, represent survivors from the bay.

Unfortunately, it was not possible to continue the work long enough to determine whether the eggs that enter past Cape Ann hatch within the bay. Although of interest, I do not think this would alter the problem, because the same current that carried them in would transport them out again.

Various investigators have shown that the fry often are transported for tremendous distances, and the few that might reach their benthonic stage during the interval between Cape Ann and Provincetown probably would not affect seriously the natural economy of the area.

It is to be expected that some of the cod eggs drifting west from the Gulf of Maine will hatch before leaving Massachusetts Bay, and there is some evidence of this. Doctor Bigelow has found cod larvæ in the bay; and Mrs. Fish, in examining two 45-minute plankton hauls (top and bottom) made by Mr. Iselin 1 mile east by south of the Boston Lightship on June 5, 1926, found 122 cod larvæ, 4.5 to 9 millimeters in length, in the haul from 8 fathoms (temperature, 39.5° F. at 10 fathoms). No larvæ were found in the surface net, but an abundance of eggs was present in both.

Certain peculiar conditions were observed early in the bay. We saw very soon that the eggs were disappearing before they hatched. We took thousands of eggs on these trips at all stages of development, but never a young cod. Two results were obvious—either these eggs were being destroyed before they hatched or they were drifting out—so drift bottles were placed at various places in the bay and at the same time the eggs were plotted. The distribution of eggs at each station and the percentage of eggs at four stages of development were taken. Later, to avoid any confusion in distinguishing between early and late cleavage and to note any line of demarcation, we divided into cleavage and embryo stages and plotted them. That showed very definitely a movement from the Plymouth Banks around the inner arm of the bay. At some times it appeared that the eggs were following close to the lower island and in others they seemed to be drifting across to other stations. The drift bottles also indicated the same drift. Of those placed along the lower arm almost all were found five or six days later either along the inner arm or in Provincetown Harbor. Those that missed the top of Cape Cod went out and divided, part drifting southeast and part to the north. Those turning to the north followed the same general direction as the bottles placed by previous investigators. It seems that the rate of drift was somewhat slower—I believe about 4 miles a day. According to the records on the return trip they did not do much more than $1\frac{7}{10}$ miles a day.

The records made by Doctor Bigelow on the movements of codfish and eggs in Massachusetts Bay give us every reason to believe that this may form a very important nursery, because the eggs may find protection and apparently ample food, the plankton communities being very rich; and it would probably be an ideal spot, not only for locally spawned cod eggs but for those passing along the whole coast of Maine to drift in and develop; and if our observations are repeated for another year they will, no doubt, show the same thing.

The solution of this problem should be sought along two lines. The eggs passing out of the bay should be followed. I have computed the rate of drift as indicated by the bottles and as indicated by the eggs. The eggs that spawned in the Gulf of Maine may drift into Massachusetts Bay and hatch. That would not seriously alter the problem, because the same current that brings them in would, no doubt, carry them out; and the few that reach the bottom in the interval might not seriously alter the general conclusions that very few remain there.

The other possibility is that a great many larvæ are destroyed in that region. In fact, some of the recent papers raise that question whether or not the larvæ are not all being destroyed in the early stages. It may be that a great many of them are being destroyed, but the cod investigation shows that the cod eggs have been hatching in the bay. The fact that so many of the pollock seem to exist there, at least for a time, indicates that they are not all wiped out.

That covers the work to date, except that there is one other little indication that at times large numbers of cod larvæ come into the bay. Some experiments were made after the conclusion of our work near the Boston Lightship by Mr. Iselin on June 5, and he found in one bottle, at 10 fathoms, 122 cod

larvæ from 4 to 5.9 millimeters in length. Eggs were abundant in both bottles. We do know that some larvæ come in at certain times, but I do not think any of them remain long enough to be of any importance, because, obviously, the drift is a permanent thing.

MACKEREL

By O. E. Sette

The mackerel investigation was begun in June, 1925. Thus we have to date only a part of the field data for the 1925 season and all of the data for the 1926 season. All of the material collected is not yet analyzed, so that at best we can present at this date only a preliminary report on limited phases of the subject.

The first work was done during June and July at Woods Hole, Mass., where the pound-net fishermen were visited as often as possible and specimens of mackerel were secured in such quantities as could be handled in the laboratory. The particular object in this first work was to determine the technical methods suitable for this species. Large numbers of fish were measured, weighed, sex determined, scales and otoliths collected, and their condition of spawning maturity determined.

It was soon evident that the mackerel caught in the traps were generally much smaller in size than those taken by the offshore purse seiners, the trapped mackerel ranging between 20 and 30 centimeters in length, while from the market reports received from Boston it was evident that larger mackerel were taken by the purse-seine fishery.

In order to cover the purse-seine fishery we secured the services of Mr. Gregersen, who took data at Boston from August to the end of the season; and during 1926 we were fortunate in securing the services of Mr. Nesbit, who collected the material of the purse-seine and gill-net fisheries throughout the entire 1926 season. These data consisted of relatively accurate information on the time and locality of capture of each vessel's catch. Measurements were taken daily on 20 to 100 mackerel selected at random from each of as many catches as possible. Scales were taken from five mackerel, selected at random from each of as many catches as could be handled daily, and such other general notes on the fishery as might pertain to the problem. During the 1926 season 1,206 vessel skippers were interviewed, over 25,000 mackerel were measured, and scales were taken from about 3,500.

Before presenting any of the data it will be necessary to describe the fishery in general, using as a basis our experience during the 1926 season.

Mackerel first appeared at the Delaware capes shortly before the 15th of April and were caught progressively farther northward until in the latter part of May, when they were taken in the vicinity of Block Island. At this time the majority of the purse seiners left the southern fishery and outfitted for the so-called "Cape Shore" fishery—that is, along the coast of Nova Scotia—where they fished during the first two weeks in June, returning with fairly good trips of mackerel about the middle of June, when they again took up the fishery in the Gulf of Maine, from Nantucket Shoals to the coast of Maine. Here they continued to make good catches until the early part of October, when the landings tapered off in quantity, and continued to do so until the middle of December, when the season finally was concluded. Summarizing, there are four natural divisions of the season:

1. The southern fishery, extending from the middle of April to the first of June and catching mackerel south of Cape Cod.
2. The "cape shore" season, from June 1 to June 15, when most of the purse-seiners are operating off Nova Scotia.
3. The "summer fishery," extending from the middle of June to some time in October, with the fleet operating in the Gulf of Maine.
4. The "autumn" season, from October to December, when the catch becomes more uncertain and the season tapers to an end with the approach of winter.

These are the natural divisions recognized by the fisherman, and, as we shall see later, they seem to have biological significance.

Referring now to Figure 17, we may present a summary of our data on sizes of mackerel during 1925 and 1926. The 1926 graphs are shown by the solid-line curves, representing the frequency distribution of sizes of mackerel in the purse-seine catch. The base lines of the various distributions are plotted with ref-

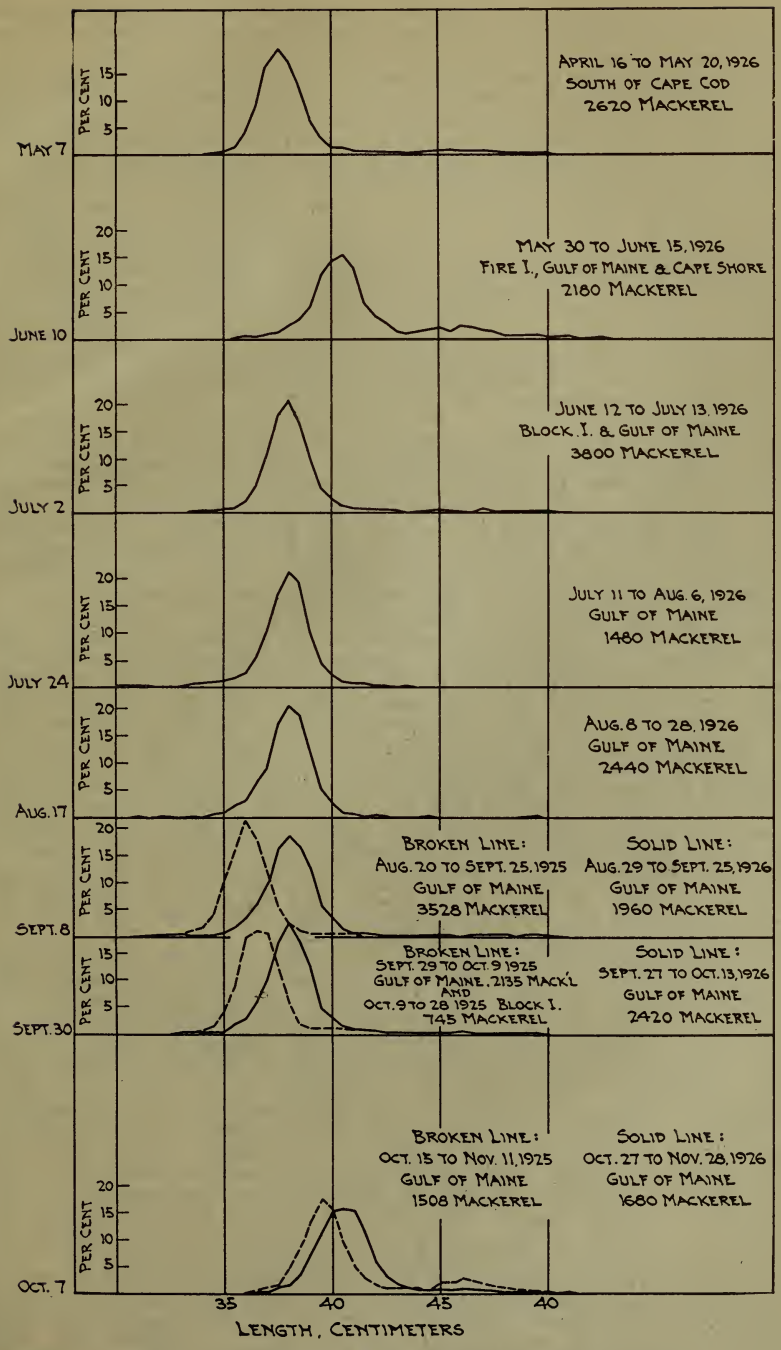

FIG. 17.—Frequency distributions of sizes (length from snout to middle rays of caudal fin) of mackerel during the seasons of 1925 and 1926

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erence to the date that approximately represents the mid-point of the time interval during which the samples were taken, this mid-point being the date of the median sample of the series taken during the interval covered.

Disregarding for the moment the second and eighth frequency distributions, it should be noted that the sizes are grouped very uniformly around the mode at 37½ and 38 centimeters in the first and third to seventh distributions, respectively. Let us call the mackerel around this mode "medium sized." Turning now to the remaining two distributions (the second and eighth), we find the concentration of sizes represented by modes at 40½ centimeters, in position with lesser concentrations at 45 and 46 centimeters. Let us call these the "large" and "very large" mackerel. With these definitions in mind it is readily apparent from the graphs that medium-sized mackerel were caught in the southern fishery and during the summer New England fishery, while the large and very large-sized mackerel were caught during the cape shore season both along the coast of Nova Scotia and along the cost of New England by the few purse-seiners who remained in the New England fishery; and that, again, in autumn the large and very large mackerel appear in the catch off the New England coast. The full significance of these data on sizes can not be appreciated without a knowledge of the relative quantities of mackerel taken in the various portions of the season. We have not yet had time to analyze this feature in any accurate fashion, but we know that in general the quantities of fish caught during the southern fishery and the summer New England fishery were far greater than the quantities caught during the cape shore season and the autumn fishery, when the large and very large mackerel were present. With this in mind, it may readily be seen that the greatest bulk of the mackerel caught during the 1926 season were of medium size, and this may possibly be as high as 80 per cent of the total catch. We believe that it is highly significant that such a large portion of the catch was comprised of the medium-sized mackerel with the mode uniformly at 37½ and 38 centimeters.

Referring now to the 1925 curves, shown by the broken lines,[15] there are certain similarities and dissimilarities between the 1925 and 1926 data, which appear highly significant. From August 20 to October 28, 1925, corresponding to the latter part of the 1926 summer fishery, we have a similarly uniform group of sizes, the only difference being that the 1925 frequency curves have their mode at 36 and 36½ centimeters, as compared with 38 centimeters in 1926. Furthermore, the frequency curves during the 1925 autumn season apparently correspond to the large mackerel of the 1926 autumn season, with their modes at 39½ instead of 40½, as in 1926. It probably is not venturing too much to assume that the shift in the modes of the medium-sized mackerel from 36 centimeters in 1925 to 38 centimeters in 1926, may be due to growth, just as the shift from 39½ to 40½ centimeters in the large mackerel may also be due to growth. If so, it would appear that the stock of mackerel from which the bulk of our catch was taken in 1925 was of the same year class as that which formed the bulk of the catch in 1926.

If this progression in size from one year to the next represents the growth of a single year class from 36 to 38 centimeters, it is not unreasonable to suppose that in 1927 this year class would have grown so that its mode would be at 39½ centimeters and would then be of similar age and size to the large mackerel, with their mode at this length, in 1925. The 1925 large mackerel have progressed from 39½ to about 40½ centimeters in 1926. Considering these facts and assumptions, it is possible to trace, tentatively at least, the growth of the mackerel from 36 centimeters to 38 centimeters to 39½ centimeters and to 40½ centimeters in four successive years. These four points, when plotted in the fashion of the usual growth curve, present a contour that suggests that they really represent the growth of the species. With the addition of the 1927 data, these assumptions probably will be verified as facts. Meanwhile, it appears that our present catch of mackerel is based, for the most part, on a single year class, supplemented in small part by a year class of secondary importance two years older.

This theory will be tested further by the examination of scales and otoliths from mackerel collected during 1925 and 1926, and these collections will be continued in future years. Age determinations of these scales and otoliths will be made entirely independently of the data on lengths, and should afford an entirely independent corroboration of the year groups tentatively discussed in this presentation.

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[15] These have been plotted on base lines representing similar parts of the 1926 season.

Should our year-class theories be verified by subsequent study, we shall have an explanation of the tremendous fluctuations that occur in the American mackerel fishery; for if, in any one year, we depend mostly or wholly on mackerel of a single year class, it means that the stock of mackerel in the ocean is augmented principally by the offspring in unusual abundance in certain years, with one or a number of years intervening when very few young mackerel survive to enter the commercial fishery. Not only would the establishment of this fact explain the fluctuating abundance of mackerel, but it would also make possible the foretelling of years of abundance and years of scarcity, which would be a service of no little importance to the mackerel fishermen and fish dealers.

The establishment of this fact may also have tremendous importance in the conservation of the species. Each successful season leads fishermen to outfit in excessive numbers for the following season, and, having incurred heavy outlay in outfitting, they are obliged to continue even after it is evident that the season is a poor one. Were it known in advance that the season would be a failure, the fishermen would, in large part, turn their attention to other fisheries, avoiding a profitless investment in outfitting for the mackerel fishery and thus relieving the species of undue strain during critical periods in its survival.

In addition to the large secular fluctuations of the fishery, which may be traced to the dominant-age classes, there are the fluctuations within the season. These raise a number of questions of utmost importance to the fishing industry, questions that a greater knowledge of the biology and ecology of the species should answer. Why are mackerel caught abundantly on some days and not on others? What causes the appearance of mackerel at certain times at certain places? Why were the medium mackerel taken in the southern and summer fisheries, and why were the large and very large mackerel taken only during the cape shore and the autumn fisheries? In an attempt to answer these questions eventually we are compiling data of the catches and temperatures at various points along the coast; and as the contributions of the oceanographers provide additional knowledge of the physical and ecological conditions in the north Atlantic, we may ultimately be successful in understanding the causes of the intraseason fluctuations.

There are many other problems that also must be undertaken along with the major part of the investigation. These are being carried on in so far as our resources will permit. We have done some townetting for mackerel eggs and larvæ and have established the fact that in 1926 there was a great abundance of these forms in Massachusetts Bay. It is hoped to continue this work in order to correlate, if possible, the years of successful spawning with the years of subsequent successful commercial fishery. It is also necessary to know whether the stock of mackerel found along the American coast is a homogeneous population, or whether it is composed of several distinct races, which might individually be affected by unequal birth rates and thus produce differing size dominance in various regions. Tagging experiments, which might throw light on this point, were initiated in 1925, but due to the technical difficulties in finding a suitable tag these were discontinued for the time being without results that we can consider significant. It is hoped to develop a suitable tag and continue these experiments, and also to examine mackerel in the various regions from a morphological standpoint to determine whether racial characters are present. Meanwhile, our data are being collected in a manner that will permit of segregation according to such localities as may be designated for separate analysis in future migration and racial studies.

Doctor BIGELOW. I would like to ask one question—why the growth of the mackerel is so extraordinarily slow after they get to a considerable size?

Mr. SETTE. That we will have to ascertain.

Doctor BIGELOW. In fact it does not grow at all.

Mr. SETTE. The growth is very slow; but the material we had was not very extensive.

Captain WALLACE. Is the temperature of the water taken into consideration in the matter of scarcity?

Mr. Sette. That is another thing I have left out. Since last year, the Bureau of Fisheries has arranged with the Lighthouse Service to supply the temperatures to these light vessels, as far as they go.

Doctor Bigelow. You do not say anything about why the towing is so successful this year and whether there were a lot of eggs in the water.

Mr. Sette. We strained the water in Massachusetts Bay from about the middle of May to early in July, towing close to the bottom and at the surface, thus getting a large number of plankton samples. However, there was no one to look over the material, so I went at it myself; spent about six weeks over it and examined only a fraction of the material, covering approximately the collections from the middle of June and extending over two weeks. I identified, beyond reasonable question, an astounding number of mackerel eggs, and during the last days the larvæ appeared in numbers that were astonishing to me.

Doctor Bigelow. Certainly, only a handful of larvæ.

Mr. Sette. I got as many as 80 to 100 larvæ in one sample, and the eggs numbered tens of thousands.

STUDIES ON LARVAL FISHES

By Marie D. P. Fish

The spontaneous remarks of visitors to the laboratory, when they peer through the microscope at our work, are not always complimentary. It is a difficult task to convince many very practical persons that a tiny thread of fish life—often just a few millimeters in length—can truly be of such economic worth that a perfectly able-bodied man or woman is justified in spending time upon it. Fortunately, I do not have to apologize to this group!

It is a knowledge of that crucial period between the time when the adult fish is indicated merely by a few cells or an egg-confined embryo, until it ceases to be carried around passively by currents and tides, that gives us a key to the distribution of species, abundance, and the answer to many such questions, which we, as fisheries' investigators, are constantly asked to solve.

My general problem has been the identification, embryology, and distribution of the young of North Atlantic fishes. I place identification first, inasmuch as other problems are dependent, primarily, upon a determination of species. The work has consisted in the identification of all larval and young fishes collected by the various fisheries vessels in the region, a study of their early life history based on these records, supplemented wherever possible by artificial fertilization and development experiments in the laboratory. It has been our purpose to study the seasonal distribution of the species that occur in this region in relation to physical factors, and particularly in relation to the other organisms that occur with them in the sea. Much of this investigation has formed a part of the problem of the early life histories of the cod, haddock, and pollock now being carried on by Doctor Fish. A complete knowledge of the life histories of all fishes forms an essential part of the bureau's program in the North Atlantic. At the present time almost nothing is known concerning the early stages of many species, some of them commonly abundant.

As to methods, the surest way to identify a larva with an adult species is, of course, to secure a ripe male and female, fertilize the eggs artificially, and study the resultant developmental stages in the laboratory. Many of the commoner North Atlantic fishes have been studied in this way, and we have excellent descriptions and illustrations based upon the experiments. However, the possibility of keeping a larval fish alive for long after the yolk sac has been absorbed and the animal is actively feeding is very slight. We can not duplicate exactly their normal conditions of life in the laboratory, and, therefore, only early larval stages can be reared.

The chief source of our larval-fish material is the sea itself. Gaps often occur in our developmental records, for certain stages can not be found by ordinary collecting methods.

In collecting young fishes we employ nets of various kinds. Silk plankton nets, small 1-foot nets at the surface, and the ½-meter and 1-meter varieties are used commonly for eggs and larvae. The Petersen young-fish trawl, especially designed for postlarval and larger fishes, is very effective. In order to determine the exact depth of distribution, closing nets are necessary, of course, but our work rarely needs such fine differentiation. It is highly desirable to examine the specimens immediately, for preservatives are apt to shrink and distort the material and to change the color.

Although the larvæ of most fishes are totally unlike the adults, and at first glance offer no clue to their identification, there are a few characteristics that are permanent throughout all stages, from the embryo to the adult. Such a constant character is the number of vertebræ and, later, the number of fin rays. In the earliest stages, before the fins are distinguishable, the vertebral count is practically the only means of identification. There are certain peculiarities for such species, especially shape and pigment markings, which make them easily distinguishable subsequently, but the first identification is possible only by counting the vertebræ.

In some tiny specimens strong light will be sufficient to reveal the spinal column, but usually it is necessary to use stain, and in larger fishes to disect. Alizarin is used a great deal, as is new methylene blue. The fish is then cleared in clove oil, xylol, or oil of wintergreen, and mounted in balsam.

These methods have been applied to certain specific problems. In relation to the cod problem, I have cooperated with Doctor Fish in the identification, description, and measurement of all eggs and larvæ, computing percentages and charting results. This work has not been confined to the cod, haddock, and pollock, but has included all other species taken with them.

An unusual opportunity was afforded by the *Arcturus* oceonographical expedition for the study of larval fishes, their distribution over large areas in the Atlantic and Pacific Oceans, the embryology, food, and enemies, the determination of unknown forms as well as new stages of known species, and the conditions of life under which they exist in the open sea.

Throughout the cruise, larval and postlarval fishes were found distributed everywhere over the ocean, but the number of species and the actual abundance of specimens were strikingly different in the various regions investigated. From Bermuda, southward, in the Sargasso Sea area, every haul of the plankton nets and Petersen trawls yielded quantities of young fishes—often 10 to 20 species at a time. Although this part of the Atlantic had been subjected to heavy storms for some weeks past, the larval fishes seemed to thrive well, if we can judge from the number of uninjured specimens taken. It would appear, also from the collections here in a region that typifies conditions of the open sea, that a great many ocean fishes spend the early part of their lives at the surface. Of 41 species that I recorded and described in late February and early March, approximately 80 per cent were found always at the surface, 10 per cent at depths of 100 to 200 meters, and 10 per cent only in nets from below 1,000 meters.

The Pacific Ocean, in contrast with the Atlantic, although swarming with animal life, yielded noticeably few larval and postlarval fishes from March until the middle of June. Although every haul brought in a few larvæ, and the number of different species represented over the whole period was no less, the total number of specimens was much smaller than in the Atlantic during late February, March, and July.

On the *Arcturus* I worked out the embryology and early development of five species of flying fishes, some of them taken from nests of Sargassum weed. The unusual modification of the fins was found to be evident even in the egg. The eggs of 15 different species, of which the development was previously unknown, were hatched in the laboratory. At the completion of the expedition I had figured and described 161 species of larval and postlarval fishes, and further study of the collections undoubtedly will reveal many more. I was fortunate in obtaining and hatching the eggs of the burrfish, of Coryphæna, and even of *Mola mola*, the giant sunfish. Although this latter species may weigh almost a ton, as an adult, it starts life as an egg only 1 millimetter in diameter.

The *Albatross* made an extensive collection of tropical larval fishes, and through my work on the *Arcturus* I hope to be able to identify many of these.

A problem in which I have been interested since 1922 is the appearance and disappearance of tropical fish migrants at Woods Hole. The Gulf Stream, sweeping northward up our coast from the Gulf of Mexico, carries with it

great quantities of floating gulfweed, *Sargassum bacciferum*, under which numbers of little tropical forms feed and live. For some hundreds of miles they travel away from their normal home but surrounded by the same neighbors, the same conditions of temperature, and possibilities of food which they experienced in the region of the West Indies.

A first glance at a list of fishes, with their distribution areas, would astonish the observer. Dozens of species, known from Florida to Brazil, are recorded from just one northern region—the vicinity of Woods Hole, Mass. Why should this spot be favored with fishes alien to the rest of the coast between Cape Cod and the southernmost States? The explanation lies in the contour of our Atlantic seaboard. Below Cape Cod the coast line runs approximately north and south, except between New York City and Chatham, where it turns sharply and runs in an easterly direction. The Gulf Stream follows this curve, so that it lies due south of Marthas Vineyard. Thus the prevailing southerly winds of summer tend to speed the rate of northern movement of the stream south of New York, while north of this turning point their effect is to blow from its course the Sargassum weed, with its attendant fauna, into the shallow waters along the shore. In summer the broad belt of water between the Gulf Stream and the shore is sufficiently warmed so that any tropical forms that follow weed blown out of the Gulf Stream can survive. Records since 1893 seem to indicate that 12.5° C. is the minimum temperature for these species. When the temperature drops below this limit in the fall, all perish. I have recorded 92 subtropical species at Woods Hole (by "subtropical" I mean those species which our temperate waters have in common with the Tropics) and 70 species of truly tropical fishes. This group comprises those species having a definite range, which extends northward no farther than Florida or, in rare cases, to South Carolina. As nearly all of these visitors are either young or of small size, it is probable that they enter local waters only in company with the Sargassum weed.

The *Arcturus* oceanographical expedition was directly responsible for the discovery of the first American eel eggs known to science. We were at station 100, about 15 miles southwest of Bermuda, when a Petersen trawl brought up four specimens. After incubation for about seven days there emerged a tiny leptocephalus, 9 millimeters long, provided with great fanglike teeth. The method of identification was by counting the myomeres, which correspond in number to the vertebræ, and the specimen was found to have the correct number for the American eel. No other species had exactly this number, and others within 20 of the count had other specific differences that allow their elimination. The specimen had come, moreover, from the very place in the Atlantic designated by Johannus Schmidt to be the breeding ground of the American and European eels. Although Schmidt had never found an egg of either species in his 18 years of searching, he concluded that they must be in this region, since it was here that he found his smallest leptocephali. My paper on the American eel is in press now as a volume of Zoologica. The preliminary notice of the find appeared in Science two months ago.

NEW ENGLAND SALMONIDÆ

By Dr. William C. Kendall

As the chairman has indicated, my subject has been listed as "Salmonidæ of New England." My letter of advice concerning this conference was something to the effect that I should speak in reference to my work within the past year or so. While, in conformity with that advice, my major subject pertains to smelts, I should like to say a word about the salmon of New England, which from the sea ascend fresh-water rivers to spawn, and the so-called "landlocked" salmon, a permanent resident of inland lakes, both of which are propagated by the bureau.

The landlocked salmon has been variously regarded as a subspecies (*Salmo salar sebago*), a distinct species (*Salmo sebago*), and by some as structurally indistinguishable from the sea salmon (*Salmo salar*). Basing my conviction upon my own studies of the fish, I regard it as a distinct species. Further than that, there is some evidence that there may be different races in different waters, as for instance, in Grand Lake in Cumberland County, but I shall not try to prove it now, although the facts are important to fish culture.

The Bureau has been conducting tagging experiments with some of the Atlantic fishes. I should like to see such operations extended to the Atlantic

salmon. A few years ago there was an unprecedented catch of young salmon ("smolts") in traps and pounds along a section of the Maine coast. The following year there was an abundance of "grilse"; later larger salmon were numerous; but last year (1926) the salmon catch was very small. For years the salmon catch on the coast of Maine has fluctuated, but until comparatively recent years salmon were taken only rarely in the outer Casco Bay region.

For many years the United States Fish Commission propagated the Penobscot salmon, and the Bureau of Fisheries has continued the operations to some extent. It seems to me that it would be worth while if the periodical appearances of salmon could be traced positively to the operations at Craigs Brook. Tagging adult salmon might be supplemented by marking some of the output of the hatchery.

Referring to my major subject: The smelt is a small species best known as a marine fish, which, like the salmon, ascends fresh-water streams (principally brooks) to spawn. As it appears in the market, it usually ranges from 5 to 7 inches in length, sometimes larger, sometimes smaller. Certain lakes contain "landlocked" varieties of smelt. My studies pertain to both the salt-water and fresh-water forms.

At one time the fishery for smelt was quite an important industry on the coast of New England and to some extent as far south as New Jersey; but it has greatly declined. It is a shore fishery still of some importance in Maine, and in value per pound the fish ranks with some of the commercially more important food fishes.

One of the objects of my work has been, by study of the biological and physical conditions affecting the supply, to endeavor to find some facts upon which regulatory measures designed to check the decline and possibly to improve the fishery might be based. While for many years the smelt fishery of Maine has declined gradually, that of New Brunswick, in the meantime, has undergone a remarkable growth, a great part of the products of which are shipped to the United States. Nearly 40 years ago the inspector of fisheries of that Province sounded an alarm concerning an imminent destruction of the fishery unless steps were taken to prevent it. Later regulatory laws were enacted, protecting the smelt in the breeding season and restricting the commercial fishery to two and one-half months in the winter.

In Maine there has never been any protection of the smelt while spawning in fresh water and they were caught at that time in great quantities, but not as a commercial fishery. Men and boys, from near and far, still visit the brooks and take all they can get almost every night during the breeding season. However, there is a close season for the commercial fishery during certain months in the year.

Maine has two independent commissions, one of which is the Inland Fish and Game Commission and the other the Sea and Shore Fisheries Commission, and each has its own laws and regulations. The general smelt law pertaining to the sea and shore fisheries prohibits the capture of smelts in tide water between the 31st of March and the 30th of September, inclusive. The Inland Fish and Game Commission's law allows the fish to be caught in fresh water, above tide water, for an indefinite period after April 1.

Before outlining the problem pertaining to the fresh-water smelt, first I will briefly describe some features of my work on the marine smelt, because of its bearing upon the situation just described and because it is the latest to engage my attention, although it simply supplements what I have done from time to time in years past.

In the last three years I have collected samples of the runs of smelts in the brooks during the breeding season of about a month in each year. My procedure was to visit the brook nightly, for the smelts run only after dark, on the ebb tide, beginning soon after high tide.

The size of the samples secured depended much upon the number of other fishers on the bank, which might be anywhere from 1 to 20 or more. The more smelters there were, the fewer fish would be caught by each. As high tide occurs about an hour later each night, of course my visits were succeedingly later. The following day the fish were measured and marked according to sex and scales were taken from each or from representative sizes for subsequent study. Three length measurements were taken—total length, length to fork of tail, and length to end of scales at base of tail—so that if any puzzling situation arose the use of one or the other dimension might remove the difficulty.

Inasmuch as the other dimensions, particularly that of the total length, are subject to more or less unavoidable variation, the most exact measurement is

from the tip of the snout to the base of the tail. However, thus far I have used only the total length, which probably is correct enough for the immediate purpose. This measurement has been used in constructing length-frequency curves using class intervals with a range of 5 millimeters.

Inasmuch as a sample may not have represented the exact proportions of the various sizes of individuals composing a run, perhaps in a way it might be regarded as a selected lot. Yet in securing the sample it was a matter of "catch as catch can," so that when all the samples are considered as a whole the combination may be regarded as at least approximately representative. The combination of samples was made at the end of each season.

All the samples collected between April 1 and May 2, 1925, comprising 735 individuals, ranged in length from 130 to 270 millimeters. It is seen that the highest peak is reached in the 170–174 millimeter class interval. There is a fairly uniform curve from the 130–134 to the 190–194 class interval. Thence the number of individuals irregularly decreases, with hardly distinguishable peaks and troughs, to 270 millimeters.

As concerns the ages represented. it is difficult to judge where each year class begins and ends. It is clear that the youngest fish are comprised in the highest curve, but beyond that the matter becomes uncertain. To determine that point I resorted to the scales taken from the individuals composing each sample. As the age was determined by scale reading, that shown by each length of fish was jotted down opposite each class interval. By this method it was found that virtually all individuals up to the 185–189 class interval were 2 years old, but along here there was some overlapping by the next age group. The remainder were 4 and 5 years of age, but the 3 and 4 year age groups overlap a little, as was to be expected.

The graphs show that increase in age is accompanied by decrease in number of individuals.

To sum up: The determination of the ages of 1,254 individuals comprising the samples represented in the graphs for 1924, 1925, and 1926, reveal 1,057 2-year-olds, 152 3-year-olds, 38 4-year-olds, and 7 5-year-olds. It seems that the dominant year class is the 2-year-olds, which were to spawn for the first time.

The great majority of smelts that are marketed are of this class, also, although there is a greater proportion of older fish, which are sorted out and sold for a higher price. Thus, it appears that the smelt fishery depends largely upon 2-year fish.

From inspection of considerable material from three localities on the coast of Maine and from one in New Brunswick, I had thought that different geographical races might be represented by them and that proportional measurements, counts of fin rays, scales, and vertebræ might yield information on that point. Good-sized collections from each of the four localities were examined. Measurements of various dimensions were reduced to percentile proportions, and these, as well as the fin-ray counts, were each treated in the same manner as described in the case of total length. In other words, frequency curves were made. While some of the proportions showed possible racial differences, the amount of material was not sufficient to prove that the differences were not attributable to some other cause, as, for instance, dominance of a different year class, in one or another collection. Scales of these fish have been studied and they suggest that possibility.

Attention was also given to rate of growth of young smelts, but I had no extensive collections covering any one entire season. However, collections in several consecutive months, from May to March following, although not all in the same year, approximately indicate the rate of growth in the first year.

Recently hatched smelts average 5.15 millimeters total length on May 13. In March the range was from about 60 to about 90 millimeters, with the peak in the class interval of 75–79 millimeters. On August 16 the peak was in the 35–39 millimeter class interval; on October 14 in the 55–59 millimeter class interval; in December in the 65–69 class interval; shown by 126, 30, and 22 individuals, respectively.

That the curve may be affected by the facts that some of the months were not in the same year is indicated by another group, representing collections of November 4, 1907, and December 11, 1918. Here the class intervals are the individual lengths of the fish. The lengths range about the same, but the peak in the curve of the November fish (about at 73 millimeters) exceeds that of December (at about 68 millimeters) by 5 millimeters. As previously indicated, 2-year-old smelts may range from about 140 to 200 millimeters.

I have used so much time already that I will only briefly refer to the freshwater smelt.

While the fresh-water smelt fishery is not extensive, and is localized, being almost wholly confined to Lake Champlain, the fish present several problems with which I have been engaged; but I will discuss only one of them at this time. This problem pertains to two apparently distinct size groups of smelts, which occur in the same lake. Other lakes apparently contain only one or the other size group. The difference in size is conspicuously manifest as the fish ascend inlets to spawn. In Sebago Lake, for example, the large form ranges in length from 8 or 9 inches up to 15 inches, or more, and averages, perhaps, around 12 or 14 inches. The maximum length of the small form is less than 6 inches and the average between 4 and 5 inches.

The principal question concerning these two forms is whether they represent distinct species, subspecies, or races, or not, or whether the small form is only the juvenile of the large form. There is some evidence that they are distinct. The breeding run of the large form occurs earlier than that of the small form. At Greenlake, Me., for example, where both sizes occur, the large form spawns in the last of March or early in April and the small form in May. The feeding habits and food of the two forms appear to differ. In Sebago Lake the large form has been found to subsist largely upon young smelts and the small form of smelt, while the small form eats Entomostraca almost exclusively.

The large form of Greenlake, transplanted in Michigan lakes, has become established in some of them, and, according to Creaser, no small form has been observed there. With considerable data on hand, an attempt was made to ascertain to what extent, if any, this evidence is supported by structural characters.

As in the case of the marine smelts, proportional measurements and counts were made in the same way. These show some differences. Thus, for example, the small form has a proportionately shorter head, the distance from the snout to end of maxillary is shorter, and it has more gill rakers than the larger smelt. A comparison of the probable errors of the means, made by Dr. Willis H. Rich, in each of these shows that they are unquestionably significant; but just what they signify is hard to say, as the apparently distinguishing structural characters of the small form may be those of the juveniles. It has not yet been observed, at least in my collections.

However, as a general rule the heads of the young fish are proportionally longer than those of older fish, and the reverse is the case in the instance in question. The large form is structurally more like the salt-water smelt, and the two have virtually the same number of gill rakers.

Comparison of the small form of fresh-water smelt with the salt-water smelts shows that in those particular characters they differ from one another in almost the same way that the small form differs from the large form; but scale readings show that the majority of the breeding small form are 2 years old, with a few 3-year-olds, while the smallest of the large form thus far examined are 3 years old.

There are other situations that complicate the problem, but those stated will suffice for the present.

The practical importance of a solution of the question lies in the fact that only the small form, with perhaps young of the large form, is eaten by larger food and game fishes. In stocking waters with salmon it has been customary to introduce smelts for salmon food in the same waters. If smelts are to be distributed thus for food for other fishes, the small form would be the better for the purpose, for in its relation to the young of some other fishes the large smelt is known to be a formidable predator in some waters. So, pending a positive solution of the problem, perhaps it would be as well to regard the two forms as distinct and to distribute only the small form; for if it is distinct the desired end would be attained, and if the form is simply a juvenile of the large form, the same results would follow as if the large form were distributed.

Mr. RADCLIFFE. I would like to ask Doctor Kendall a question. He called attention to the fact that there is no protection for smelt in the breeding season. Would it be possible to take smelt in any other season?

Doctor KENDALL. That is when the commercial fisheries are carried on outside of the breeding season.

Mr. RADCLIFFE. Carried on outside the breeding season?

Doctor KENDALL. About the middle of September or the first of October the season begins and continues until March.

Mr. Radcliffe. Is the catch of small smelt?

Doctor Kendall. Six or seven inches long.

Doctor Gilbert. In fresh water?

Doctor Kendall. There is no commercial fishing in fresh water.

Mr. Radcliffe. You are speaking of protection for the breeding smelt?

Doctor Kendall. There is no protection in the breeding season. Great quantities of young smelt that have not attained 1 year of age are caught in the fisheries of Casco Bay, and I presume the same fisheries see that these little fellows, only a few inches long, are brought into the market when the larger smelts are not present in sufficient quantities. I went into Shores's grocery store one December and he had some smelts he was selling for 30 cents a pound. I bought a pound of them to examine. There were 41 smelts in that pound. In the same month, in Massachusetts, I bought 1½ pounds of smelts and paid 75 cents for them. There were just 10 smelts in the 1½ pounds. The small smelts certainly need protection.

SOUTH ATLANTIC AND GULF FISHERIES

WORK OF THE FISHERIES BIOLOGICAL STATION AT BEAUFORT, N. C.

By Dr. S. F. Hildebrand

Of all living animals, none remind one quite so much of the giant, clumsy, lumbering reptiles of prehistoric times as do the turtles, tortoises, and terrapins. Some of these animals, such as the leather turtle and the loggerhead, still reach a very considerable size. The diamond-back, however, is small, for it seldom exceeds 7 inches in length when measured on the median line of the lower shell. In sluggishness and awkwardness it is quite the equal of its larger relatives, and ample evidence is available to show that this lowly creature, which nevertheless possesses meat that is unexcelled in flavor, is doomed to meet the fate of its ancient relatives unless man, who is its chief destroyer, becomes less destructive and, on the other hand, comes to its rescue by means of artificial culture. The rescuer and the perpetuator of the diamond-back terrapin has appeared in the form of the United States Bureau of Fisheries, assisted by the fish commission of North Carolina.

The work at Beaufort concerned with the propagation of the diamond-back, as stated in the last report of the commissioner, has definitely passed the experimental stages, and practical cultural operations have been begun, thanks to the aid given by the State of North Carolina.

There are on hand at the Beaufort station at the present time about 3,330 adult terrapins, consisting of 2,610 females and 720 males. The State has placed 1,760 adult animals there, consisting of 1,320 females and 440 males. All of the other breeding animals belong to the experimental lots of the bureau, and all of the last-mentioned animals, exclusive of about 370, have been grown in captivity.

About half of the animals belonging to the State were received too late for the last breeding season, and the others had not been in confinement long enough to become fully acclimated. Quite a few of the bureau's animals, included in the number previously given, are only just maturing. Reproduction, therefore, for the past season, considering the large number of adults on hand, was very low, as only about 4,360 young have been uncovered to date. Only 700 of these are the offspring of the animals owned by the State, all others coming from the experimental stock. It is expected that the hatch will be doubled next summer and that in another year it will climb to 12,000 or even 15,000, and that it will continue to climb as more of the bureau's animals mature and the State terrapins become better acclimated, and that a hatch of 25,000 to 30,000 will result by 1930.

Even the largest number mentioned is negligible as compared with the number that the fish culturist counts. Numbers hatched and liberated, however, may mean little or nothing; the number of animals that are liberated and

survive to reach the dinner plate is what counts. It is here that we believe we have a very great advantage over the fish culturists, for we are able to keep the young animals, with comparatively little work and at small expense, until they have reached a length of 1½ to 2 inches, when they have definitely passed through the most critical stage of life. At this size they have developed a hard shell, are able to get about more rapidly, and they are no longer the utterly helpless creatures they were when first hatched. They no longer can be destroyed by mice, small birds, crabs, or small fish. We are able to bring the animals to this point within about six months, when winter fed, and within a year and six months if permitted to hibernate. The loss among these young animals seldom has exceeded 15 per cent, which is a lower mortality than commonly occurs among young chickens.

Furthermore, we have some evidence that a fair percentage of the animals liberated live and grow in the wild state, for of the comparatively few (consisting of only several hundred) individuals liberated at Beaufort each year, from 1914 to 1923, several have been recovered; and the rate of growth in these animals compared very favorably with those of the same age kept in captivity. It is believed, therefore, that the diamond-back terrapin cultural work along the lines now pursued at Beaufort promises to bear fruit from fifty to seventy-five if not one hundred fold.

When it was said that the work had passed the experimental stage it was not intended to leave the impression that experimentation had ceased. There is much to learn, and at least 10 definite sets of experiments, involving 21 lots of terrapins, are now under way. As time is less of an item in the life of a terrapin in North Carolina than to the proverbial hog in Louisiana, some of the experiments will have to run a long time before definite results may be expected. For example, several years ago, as the result of a conference with a geneticist of the Bureau of Animal Industry, a series of breeding experiments was planned, and the animals to be used were segregated in the spring of 1923. We have yet to wait another year before we can be sure that the offspring is from the particular males selected for the experiments. Then we will be obliged to wait at least six years longer for this offspring to mature and for the second generation.

The slow growth and the long time between generations naturally brings the question to mind: How long does a terrapin live? Or, what is its normal span of life? This is a question that does not yet admit of an answer, and I am not sure that we will live long enough to learn definitely, unless a genius should appear who can find a more reliable way of reading the age than by the growth rings on the carapace.

A lot of about 370 wild terrapins is on hand. The lot is composed of a few animals secured in 1902, others that were obtained in 1909, and the rest in 1912. Only about half a dozen of these animals have died during the many years they have been in confinement, and they were all adult breeding terrapin when secured. The only certain indication of old age of some of these animals is the smoothness of the shells, for the growth rings have disappeared, quite probably due to wear. For several years in succession egg production fell off, and just as we were becoming convinced that "old age" had overtaken them, this lot of old breeders came back last season with about the largest number of eggs and young ever produced. Consequently, we will have to guess again.

Other experiments consist of cross breeding two species of diamond-backs, comparison of rate of growth of hybrids with animals of pure blood, comparison of growth of the offspring of wild terrapins, that have been confined, with that of animals grown in captivity. Other experiments have to do with space requirements of the animals, the proportion of males to females necessary to insure fertility of the eggs, and the natural sex ratio. Among all the groups of animals that have been grown in captivity the males are very greatly in the minority. These lots, however, all consist of selected animals. Two unselected lots, each consisting of 300 terrapins, have now been set aside and will be retained until the sexes become distinguishable.

Winter feeding of young animals is being continued. One of the practical results derived from this work already has been referred to, for it was shown that a year's time may be saved in bringing the animals to a size at which it is thought safe to liberate them. Similarly, a year's time is saved in bringing the animals, when held in captivity, to sexual maturity and to marketable size. Our data indicate that a somewhat larger percentage of the animals survive when handled in this way, than if allowed to hibernate.

Necessity is the mother of invention. Similarly, many other things that are not inventions are learned when necessity knocks at the door. Last summer we were handicapped for suitable out-door space for young terrapins that had hibernated. It was decided finally to place them in the hothouse temporarily, as that place was vacant, the animals that had occupied it having been liberated. It was known, however, that the extreme heat from the glass roof would kill them unless some protection were provided. This was accomplished by coating the glass heavily with lime and by placing a board lengthwise over each tank, providing at least some shade at all times. This situation proved so successful that the animals were left there until they had to be taken out to make space for a new brood. Better growth than usual resulted, and the death rate was considerably lower. It is planned to repeat the experiment.

I have already stated that time is no item in the life of a terrapin. Therefore the year's growth gained by feeding the young the first winter probably is of no great importance when the animals are to be liberated. In practical terrapin farming this is of value, as the turn-over is shortened. The house that we have at Beaufort is large enough only for experimental purposes. At the present time we have crowded the animals very closely in some of the tanks and are holding as many as 100 to 125 in compartments scarcely 2 feet square. This is much closer crowding than has even been thought permissible. It is too early to know what the result will be. I can say only that so far the crowded animals appear to be getting along just as well as the less crowded ones. Under the largely "overcrowded" conditions, space was found for only about 2,500 animals. This number, within a few years, will be only as a drop in a bucket when compared with the large number of young that will be hatched. I am not prepared, at this time, to recommend the construction of a larger house, because I have not had time to study carefully all the data on hand relative to the difference in the death rate among the winter-fed animals, as compared with the hibernating stock. Deaths undoubtedly are fewer among the winter-fed animals, but how great the difference is can be determined only after an analysis of the extensive data is completed. This is what I regard as important, rather than the year's growth gained through winter feeding.

All the results concerning winter feeding through the years 1911 to 1925 are clouded by one factor—namely, heat. During all these years the house has been heated by means of a stove. It is utterly impossible to supply uniform and even heat all over the house at all times with a stove. Almost invariably, and largely without regard to the treatment given or the kind of food supplied, the greatest amount of growth has taken place among the terrapins nearest the stove, and the least growth has occurred in those farthest removed from the heat. This appears to show that a uniformly high temperature is highly desirable. The diamond-back is a cold-blooded animal, and without doubt digestion stops when the body temperature falls below a certain degree, as has been shown, experimentally, to be the case in a fresh-water relative. When digestion stops, it follows that growth also ceases. The interpretation of the results of virtually all of the many experiments performed in winter feeding is difficult, as it is impossible to know how much to charge to the important factor—heat—and how much to the treatment given, food supplied, and to other factors. A better heating plant is absolutely essential for the conduct of further experimental work in winter feeding.

Very extensive data on diamond-back terrapin culture that have not been analyzed properly have been accumulated at Beaufort. The working up of these data is the job immediately in hand.

FISH IN RELATION TO MOSQUITO CONTROL

We have heard much about the use of fish during the past few days. One important use has not been mentioned, however. I am referring to the employment of fish for the control of mosquito breeding. I am thoroughly convinced that fish are so important in this connection that many regions now occupied by man would be entirely uninhabitable were it not for the degree of mosquito control provided by fish. Such prosperous cities as Wilmington, N. C., and Savannah, Ga., for example, would never have been built had it not been for the measure of mosquito control provided by fish, I am as sure of that as I am of anything. Yet when fish are spoken of, few people think of these animals in that connection. In fact, it is only very recently that the value of fish as agents for mosquito control has been known. How unusual it is to think of fish in this connection I can best illustrate by telling you of a

little incident that happened a few years ago in a Southern State. I called on a State health officer, and while there a reporter came in and said to the officer, "Do you have anything for me to-day?" The officer said "Yes," and gave him the following: "Mr. Hildebrand, of the United States Bureau of Fisheries, is here conferring with the State health officer and the sanitary engineers as to the best use to make of fish for malaria control." The evening paper printed this item of news with the following headline: "Eat more fish, best way to prevent malaria."

The use of fish for mosquito control is well established in the South. Of course, different situations are met in nearly every locality, and certainly they differ among localities. In some of these a very high degree of control is brought about by the fish alone (seldom 100 per cent control results); in other environments the degree of control is lower. Intelligence must be exercised in the use of fish for mosquito control, just as in using drainage, oil, or Paris green for controlling mosquito breeding.

I was asked several years ago by a Kiwanis Club in a southern city to talk to them on the use of fish for mosquito control. During my talk I made the statement that top minnows first were used for this purpose on a large scale in 1918—that is, during the war. My statement was challenged by an engineer present, who informed us that his company used the fish on a very large scale as early as 1913. I was obliged to admit that I knew nothing of this early work. However, I made diligent inquiry later, and I learned that this power company had received, presumably through the Bureau of Entomology, half a dozen cans of Gambusia from Louisiana to plant in an artificial lake almost on the Atlantic border, which had an area of several thousand acres. The fish certainly were given a wide range and a very large scale of work to do, but that obviously was not what I meant, nor was that intelligent employment.

Several Southern States now have regulations requiring the establishment of ponds on or near the area to be flooded for the purpose of propagating Gambusia, not by the hundred or even thousands, but by the millions. Such a regulation is not difficult to enforce, for the power companies have suffered such heavy losses from damage suits brought by people living in the vicinity of newly flooded areas, who have claimed damages because they suffered greatly from malaria. Consequently, the companies have learned their lesson and they are most anxious to do what they can to prevent malaria. The propagation of the minnows, particularly under artificial feeding, even in new ponds, has been very successful, and the results derived by way of mosquito control have been good. Of course, other methods of control, too, have to be used frequently in newly impounded waters. The little fish, if plentiful enough, will destroy the wiggle-tails if they can get them. Frequently, however, floating débris is so thick and so abundant in newly flooded areas that aid must be given.

Recently, upon the suggestion of a United States Public Health Service officer, I addressed a series of letters to 14 State health officers who are doing mosquito-control work for the purpose of controlling malaria, making inquiry relative to the use of fish (Gambusia) for malaria control. One State replied that the fish had been tried and found entirely unreliable and their use no longer was recommended. Another replied that other methods of control so far had appeared preferable for the very limited amount of work done in that State. However, plans were being made to use the fish in new work planned for next season. The other 12 all replied that they considered the fish very useful and depended upon them a great deal.

Sufficient investigations have been made in the South to have determined that Gambusia is the one fish to rely upon, as it is a natural mosquito eater. We are less fortunate, however, in the North. I wish to bring to your attention the crying need for further investigations of this problem in the northern waters, where Gambusia does not occur naturally. Prof. J. Percy Moore made a good beginning when he studied this problem briefly in the vicinity of Philadelphia and New York City. However, the study should be carried on much further; it should be done more thoroughly and it should be carried to other localities. The need for such a study has been impressed upon me very strongly during the past fall, when a great many inquiries relative to the use of fish for mosquito control were referred to me by various public health officials as well as private citizens. Of course, under the circumstances definite recommendations are impossible.

We have been telling these people that Gambusia could not be used. Now I learn that this fish has lived and multiplied for several years in certain ponds

in a suburb of Chicago. This introduction was made from southern Illinois. Possibly a cold-resisting strain of Gambusia has been found, and it may be possible to use Gambusia in northern waters. In any event, the matter seems worthy of further study.

Doctor Hildebrand. I would like to add just a few words on some other work we are doing at Beaufort. Mr. Towers will present a paper dealing with the pigfish. We have followed the development of the egg and the young of the large anchovy, and we have taken up a third species of which we have the egg, but are not certain we have the egg linked with the right fish. We think it is the young sheepshead.

When we went to Beaufort we finished quite a bit of work already on hand. We had a collection of fish from Mississippi and from the Gulf of Venezuela. I had prepared some notes on Gambusia and have just completed the paper. We shall get all of these things out of the way and will then confine ourselves to the local fauna. No extensive work has been done at the station heretofore in the winter time, but we have undertaken some investigation to find out what lives in the deeper waters there in the winter.

We got many of the younger food fishes there until we had some very cold weather during the holiday season. Thereafter we got practically nothing, showing that these fish at least leave the harbor in the winter.

Doctor Gilbert. Do you remember when the Gambusia was taken to the Hawaiian Islands? That was very successful.

Doctor Hildebrand. Yes; I understand it was. It has been introduced since in the Philippine Islands and has found its way to southern China and Japan, Italy, and Spain. A shipment was sent to the West Indies recently.

Doctor Rich. Was a shipment sent to Palestine?

Doctor Hildebrand. Yes. It got across successfully, but the fish are not doing well. They have too many enemies. They still have a brood stock, but it isn't multiplying.

Mr. Higgins. The proposal for extending the work on mosquito-eating fish to the Northern States should properly be brought before the advisory committee when it considers plans for the future. I want to ask now about the success of your towing operations for eggs and larvæ of fishes in both inside and outside waters.

Doctor Hildebrand. That work has not been carried on long enough to have yielded results. The weather has been so bad lately that very few trips have been made. We have had a season of rough weather, with high winds and it has been almost impossible to do anything. I think only two trips had been made when I left Beaufort, but the work is being continued.

EMBRYOLOGY OF THE PIGFISH

By Irving L. Towers

As is probably already well known, the pigfish (*Orthopristis chrysopterus*) is one of the important food fishes of the Beaufort region. A study of its feeding habits and life history, especially during the early stages of its life, has been commenced with a view to obtaining further information concerning the type of environment best suited to its needs, the rate of growth, the character of its food, and also the outlines of its embryology.

A plentiful supply of eggs of the pigfish was secured last May with a tow net in the channel between Pivers Island and the mainland; and as this is within 100 yards of the laboratory, the eggs could be examined with the loss of but little time.

Towings made shortly after dusk yielded the best results in furnishing eggs in the early stages, it being evident that spawning takes place principally at this time of the day. The eggs are comparatively large, being nearly 1 millimeter in diameter; they are semitransparent and contain one large oil globule.

Cleavage of the germinal disk takes place rapidly, so that the 2-cell stage is reached within half an hour, the 4-cell stage at 45 minutes, and the 8-cell stage at 1½ hours after fertilization. After 24 hours several features of the embryo pigfish are discernible, including the snout and eyes, with the dorsoventrally flattened body, consisting of about 30 somites, extending around about one-half of the inner circumference of the egg. At 36 hours the embryo has assumed a more cylindrical form and has grown so as to encircle almost completely the inner circumference of the egg. The heart and the auditory vesicles are quite evident. During this process the yolk, of course, has decreased in bulk, so that at this stage it is perhaps only two-thirds its original size.

The period of incubation in this species is about 48 hours. Upon emerging, however, the young is considerably handicapped by the large elliptical yolk sac, which is equal to one-half the total length of the larva. The young pigfish, nevertheless, is quite active at this stage.

At about 2½ days after hatching, or at 4½ days after the egg was fertilized, the young pigfish had attained a length of 3 millimeters, the pectoral fins had appeared, and several characteristic chromatophores were evident. The yolk was virtually absorbed, with the exception of the oil globule, which was less than one-half its original size. The median finfold extended from the nape completely around the body, terminating at the throat.

The young, up to this stage of 3 millimeters in length, had developed in a glass dish in the laboratory, but due to some unknown cause several lots of fry expired upon attaining this size and none was carried to the next stage in the laboratory. The next stage available for study had attained a total length of 7 millimeters. These presented a considerably altered appearance, when compared with the preceding stage; the continuous median finfold had disappeared and was replaced by distinct dorsal, caudal, and anal fins, all with fairly distinct rays. This stage, however, scarcely resembled the adult in a single feature, the profile being concave, with the pineal fontanelle quite evident. Young of this class were found to frequent the more protected shoals around Pivers Island, where protection was provided by a close marginal growth of sedges.

This stage, represented by the concave profile, is very transitory, as fry only a few millimeters longer have a notably convex profile and the pineal vestige has entirely disappeared. The fins in these young pigfish of 11 millimeters retained virtually the same form as those of the preceding stage, with the exception that the spinous portion of the dorsal had commenced to make its appearance in the form of several tubercles somewhat anterior to the soft dorsal, which was already formed through a modification of the original median finfold.

In fish having a length of 15 millimeters the spinous dorsal is well developed, but there is a noticeable interval between it and the soft dorsal, although in mature fish these fins are joined. Scales have not yet been developed; the myomeres are quite distinct.

Young 1 inch in length may be recognized easily as pigfish, differing from the adult form principally in having enlarged eyes and a broad lateral stripe.

The ingested material in 100 young, ranging from 10 to 93 millimeters in standard length, was examined for the purpose of gaining some idea as to the progressive change in diet as the fish increase in size. Young of from 10 to 11 millimeters in standard length were the smallest in which the stomach contents could be identified definitely. Several of such fish had taken a few very small copepods. These minute crustaceans continue to be taken in considerably increasing amounts until the fish attains a length of approximately 30 millimeters, when chætopods are taken, after which the copepod becomes less evident and seldom is found in specimens over 60 millimeters in length. Other small crustaceans, including Gammarus, ostracods, and Mysis, supplement the copepods in the diet during these early stages; and even the larger sizes, up to

90 millimeters, continued to utilize Gammarus. Minute periwinkles appear to be taken also whenever they are available, as several fish 1 inch in length contained over 100 of these small mollusks. Chætopods, which are first utilized when the pigfish is a little over 1 inch in length, increase in frequency, forming the major part of the diet for all fish from 30 to 90 millimeters in length. The study has not yet been continued on fish of greater sizes.

Doctor Bigelow. May I ask Mr. Towers whether he has made any determinations as to the age which the pigfish reaches?

Mr. Towers. They average between 4 and 5 inches in length. I haven't any idea as to the maximum age.

Doctor Bigelow. There was some indication that they were rather a short-lived fish.

MULLET

By Elmer Higgins

For the sake of completeness, the mullet investigations have been included in this program. I shall take but a short time to outline the problems presented by the mullet fishery, without giving detailed data in support of the observations made or the conclusions that may be drawn. The work is still in a preliminary stage, due to various circumstances, although many field data have been collected. Much of the credit for this work belongs to several assistants who worked with me.

The mullet fishery of the South Atlantic States is one of the most important sources of sea food in that region. The mullet occupies the same position in the South that the codfish occupies in the North. It is eaten fresh and salted in great quantities, and in the latter condition is a staple food of the poorer people from New Orleans to Norfolk. The gray mullet (*Mugil cephalus*) ranges from New England to Brazil, but the important centers of production are in North Carolina (which produces from 1,000,000 to 6,000,000 pounds annually) and Florida (with an annual yield of 30,000,000 to 40,000,000 pounds). While two species of mullet occur in the commercial catch, the silver mullet (*Mugil curema*) is practically negligible, and for that reason attention has been paid solely to the gray mullet.

The conviction that the mullet is suffering depletion in the Southern States has been growing among the fishermen and others for many years. While it is difficult to demonstrate a heavy decline in the yield in Florida, the total yield in North Carolina unodubtedly has fallen off to a marked degree. The maximum yield in that State was reached in 1902, when 6,750,000 pounds were landed; and from that year until 1918 the trend, as shown by statistics collected by the bureau, has been constantly downward, until the total catch in 1923 reached the minimum of 1,250,000 pounds. The yield in Florida since 1890 has been generally upward, although great fluctuations have occurred. It is apparent from the figures of the various sections of the coast that the fishery in Florida is composed of several units, and hence is subject to independent variations in yield. In general, the trend of the yield on the east coast and on the southern section of the west coast has been upward, but the trend of the yield on the western extension of the Florida coast has been downward since 1897. Here, again, however, the statistics have been quite inadequate for detailed examination, and as they are taken at such infrequent intervals they may, indeed, be misleading. Nevertheless, the popular idea is firmly fixed, particularly in North Carolina, that depletion is occurring. This notion is based partly on the declining yield and partly on the fact that grossly wasteful and destructive methods have been practiced in the fisheries. For example, for many years it was the practice to seine great quantities of very small mullet for use as fertilizer, either spreading them directly on the fields or sending them to the menhaden reduction plants. Both purse seines at sea, and haul seines in the rivers and estuaries, were employed. The popular opinion is supported still further by the fact that formerly the bulk of the yield consisted of fish in spawning condition, and in some places the roe fish, too large to market profitably, were taken solely for the roe, which has sold, when dried, for as much as $1 per pound.

Because of the importance of the fishery, the growing interest in conservation, and the popular belief in depletion, supported by statistical evidence and the personal experience and testimony of old fishermen, the problem of conserving

the mullet fishery is considered acute in North Carolina, and has, therefore, received first attention in that State. As a basis for effective regulation of the fishery, observations on the life history of the mullet have been undertaken, and I shall outline for you briefly what is known from popular knowledge and from my own observations of the habits of this species.

Only two brief papers on the biology of the fish have appeared in America, and recently some observations on rates of growth and similar facts concerning an allied species have been published in Egypt; but it is surprising how little attention has been paid to the mullet, despite the fact that it has been cultivated in foreign countries for ages.

The mullet is a shore species, living in rivers and estuaries where an abundant growth of eel grass on muddy bottoms is found. In North Carolina the mullet scatter over the tide flats of the extensive brackish and salt-water sounds, feeding largely on the bottom mud and its contained organic matter. During the autumn months the scattering individuals congregate in schools and migrate from the sounds to the open sea. It is at this time that the commercial fishery takes toll of the migrating schools, which are packed so densely that many thousands of pounds are landed at a single haul of the beach seine; such as, for example, a single haul made at Beaufort in October, 1926, which yielded 62,000 pounds. The schools are composed of fish singularly uniform in size, usually of a single age class, although in the larger fish several ages undoubtedly are mingled. The early fall runs, beginning in August, usually are composed of the younger fish—the 0 class—with occasional schools of the 1-year-old fish. In September the usual runs are of 1-year-old fish, known as "fat mullet," because the abdominal cavity is filled with rolls of white, fatty tissue. Following these come the older fish, or roe mullet, which dominate the fishery after the middle of October. Virtually all of the mullet have left the sounds by early November, and it is at this time that spawning is believed to occur in the mouths of estuaries and in the open sea. The fish apparently do not remain in the open sea during the winter. The schools are observed traveling southward and entering the inlets again, and it is known that many of them either remain in the sounds or return during the late fall, for occasional catches are made during the cold winter months farther up the rivers in fresh water.

These fall migrations always accompany a change in weather, marked by falling temperature and northerly winds. Fishermen believe that the schools run before the wind out of the sounds, for they have been observed to return to the sounds if the wind suddenly shifts to the southward. Inasmuch as the younger fish take part in this migration, it can scarcely be considered a spawning migration; but as it comes at the time of the year when the eggs reach maximum observed maturity, there may be close relationship between the two phenomena. At present no spawning fish have been observed, nor have the eggs been taken in tow nets, but it is believed that proper means will reveal the presence of eggs in the open sea. As the fish with evidently mature eggs are taken on their seaward migration at this time of year, spawning may be said to occur during November and December; and it is likely that the spawning season is of limited duration, as the eggs are of approximately the same development throughout the ovaries of the fish, and as the young appear the following spring in very uniform size groups.

The larval stages of development of the mullet are unknown, but juvenile fish appear in the sounds about Beaufort in early February, when they are from 18 to 25 millimeters in length. Fish smaller than this size are seen rarely, but the group increases in abundance and the larger members of it become more numerous during the latter part of April, and by May even the smaller sizes show distinct growth. It is during April that the so-called "metamorphosis" occurs. The fish that formerly were silvery become pigmented over the dorsal areas. They are also said to change from surface or midwater plankton foods to the bottom foods, and distinct growth in scales can be observed. The circuli on the scales, which in the juvenile condition are transverse, now begin to grow in a more nearly concentric form, inclosing the juvenile scale so as to show a typical winter check. Scales from collections of these small fish made during early March had no marginal circuli, but a collection taken on March 24, 1925, contained three fish with one or two marginal circuli. Another collection, on March 30, contained a few in this condition; but by the middle of April the scales from all the collections of fish of this size group showed varying numbers of marginal circuli inclosing the so-called "juvenile scale." The mean length of this size group increases rapidly in May and maximum growth occurs.

during June and July. In the latter part of the summer the growth rate apparently falls off somewhat, but the exact figure has not been determined because of the admixture of another class of fish known as "cape mullet."

The next larger size group, which is found in the sounds during the winter and spring months, ranges from 110 to 180 or 190 mill meters in body length. The mean size does not increase until the middle of April, when growth is observed in this group, also. By the end of April spring growth has begun in this group, and the same formation of a winter check (by the inclosing of the scale by concentric circuli) is noted. At this time the fish bear on their scales a distinct winter check, which in every way is comparable to the check inclosing the so-called "juvenile scale." It is the second winter mark, therefore, and is formed when the fish is approximately 17 months of age. I have called all the fish up to this time the 0 group, and those after the formation of this winter ring, at a size of approximately 150 millimeters, the 1 group. The 1 group grows rapidly during May and at a maximum rate during June and July. By August these fish have reached a modal size of 265 millimeters body length and then appear in the commercial fishery as small mullet. In September they are mixed with the smaller fish of the next older year group, when they are termed "fat mullet." They are also taken in reduced quantities during October and November when mingled with the spawning fish.

These fish are considered resident in the vicinity of Beaufort, N. C. They are present in the sounds throughout the year, and the observed progress of the modal size is so constant that there can be little doubt of the real identity of this class of fish. In late August, or sometimes in September and October, another class of fish, known as "cape mullet," appears in the sounds. They are caught in greatest abundance by the menhaden purse-seine vessels while at sea in the vicinity of Cape Lookout before they enter Beaufort Inlet. They are taken in considerable quantities, also, by haul seines situated on the beaches inside the inlet, and southwestward along Bogue Bank. These cape mullet are distinguished chiefly by size, which ranges somewhat larger than the local 0 class in the fall and considerably larger than the 0 class in the early spring—that is, from 140 to 200 millimeters, with a modal length in September of 180 millimeters. They are also of the 0 class—that is, they bear only the juvenile winter mark on the scales. When last seen in October, however, they are nearly 50 millimeters larger than the 0 class in the early spring, and hence form a group that lies between the sizes of the local 0 group and the local 1 group. According to popular belief, they come from the north and are occasionally referred to as Virginia mullet. They certainly enter Beaufort Inlet from the sea, and are most abundant in the ocean outside the sounds. Their origin and the part they play in supporting the stock in this region are still a problem.

North Carolina fishermen believe that the migrating schools of mullet that leave the sounds in the fall proceed southward along the coast, always swimming, as they say "with their right eyes to the beach," and proceed to Florida. As dense schools are seen leaving the sounds in the fall and none are observed returning in the spring, the fishermen believe that the mullet remain in Florida and never return to North Carolina. This view, in part, is supported by Jacot, who made a brief investigation of the mullet at Beaufort during 1914–15. He believed that the mullet migrated to Florida in a leisurely fashion, feeding as they went, but that they returned in the spring singly, making a continuous voyage, and that the strain of this travel caused the formation of the winter check. His idea, however, was not supported by observations, and we have shown that the winter check is not formed by migration but by normal spring growth, which begins when the water warms to about 20° C.

This idea as to the one-way migration to Florida has had serious effect in opposing any regulation of the fishery in the interest of conservation, for the fishermen argue that if the mullet are not caught in North Carolina they will be lost to the State and will be caught in Florida. We therefore determined to examine the evidence of such migrations and whether or not the stock of mullet in North Carolina and Florida consists of a single intermingling population. Two methods of study were employed—the first by searching for morphological characters that might serve to prove the identity or the segregation of stocks in both States, and the second by tagging the fish. Numerous measurements were made of physical proportions of Florida and North Carolina fish, and it was found that the ratio of head length to body length of fish of the same size from the two localities was distinctly different. The same was true of the length of snout and eye, but the width of the interorbital was

the same in both localities, as was also the position of the fins and the number of vertebræ. These measurements show that body proportions of Florida and North Carolina fish are different to a degree of high statistical significance, and it therefore indicates the segregation of the stock and the lack of extensive intermingling

While such evidence, if sufficiently extensive, would be convincing, it was determined to make the proofs more satisfactory through tagging experiments. Accordingly, approximately 5,000 fish were tagged in the vicinity of Beaufort during 1925. A reward was offered for tagged fish, and 36 fish were returned. Early recoveries were from fish moving northward from Beaufort into the sounds, away from the ocean; during midsummer (August and September), recoveries were from the vicinity of Beaufort or to the southward and seaward; but from December to March occasional recoveries were made in the fresh-water rivers northward and inland from Beaufort, or in the same situations to the southward. The most southerly recapture was from near Georgetown, S. C., not more than 170 miles from the place of liberation.

During 1926 an additional 1,000 mullet were tagged, with almost identical returns, except that a larger proportion was recaptured. Thirty-seven were returned—three from the northern section of the South Carolina coast and all the others from North Carolina waters. It seems certain, therefore, that any decline in the North Carolina fishery can only be due to local conditions, and any restriction of the commercial fishery would result in benefit to the local stock of fish.

The question of regulation of the fishery is extremely difficult, even if it were proved that overfishing is the cause of the shortage of the supply. Natural fluctuation in the abundance and composition of the stock may be extremely important. The State collects no records suitable for the study of these changes, but our limited observations show that great variation occurs from year to year. For example, in 1925 we collected records of the landings from all of the dealers in Carteret County, where the majority of mullet are landed. The total yield did not greatly exceed 750,000 pounds. A high percentage of these were roe mullet; that is, fish 2 or 3 years of age. The younger fish were relatively scarce, and cape mullet were present only in moderate quantities. In 1926, however, a canvass of the State showed a yield of approximately 5,000,000 pounds, which contained a surprising amount of the first-class and of cape mullets. Repeated catches of 40 to 50 tons each were made by menhaden vessels, and these consisted entirely of cape mullet. In 1925 the examination of the market landings indicated that after October 10 all of the fish taken were roe mullets. The proposal was made that closed seasons be established after about October 15, which would reduce the season's catch only about 25 per cent but would result in saving more than half of the total number of spawning fish taken that year. During 1926, however, such a closure would have curtailed the total yield considerably more, and the number of spawning fish protected would have been insignificant.

Since the spawning fish are taken almost exclusively in haul seines and gill nets within the sounds it may be possible to protect them without restricting the sea fishery for cape mullet. On the other hand, free exploitation of the cape mullet may have serious influence on the supply of fish if it proves true that the cape mullet are not a distinct stock but are merely members of a rapidly growing group of North Carolina fish. It is anticipated that evidence on the subject will be secured through the detailed study of the large collection of scales now on hand, and it is hoped that means for collecting more accurate statistics in North Carolina can be instituted in order to study the variation in abundance of fish in the sea. If the press of administrative duties in this office permits, I shall look forward to rounding up many of these problems during the coming year.

TEXAS FISHERIES

By J. C. Pearson

The condition of the marine fisheries of Texas has produced a curious frame of mind among fishing interests of that State. While many insist that the peak of production has been passed long ago and that the fisheries are in a state of sad depletion, others maintain that the peak has not even been reached and that the fisheries are in a state of arrested development.

The fact that strong opposition has arisen in Texas against the modern type of fishing gear stands out clearly, however. As is usually the case this opposition comes from two main sources—from the commercial fishermen, who use less costly and less effective gear (in this case hooks and lines), and from the sportsmen, through legislation enacted for the alleged purpose of conservation.

During the summer of 1925, Mr. Higgins and Mr. Lord, of the United States Bureau of Fisheries, attempted to secure data relative to existing conditions, and this information is embodied in a preliminary report on the marine fisheries of Texas, a publication just off the Government press. In this report the authors stressed the need for more exact knowledge of the biology of the important food fishes of Texas before rational conservation needs could be realized.

It has been my privilege to conduct studies on the life histories of the leading food fish since last April. Our plans originally called for a study of the fishery itself, with careful emphasis laid on the size and age composition of the catch. This was found to be impracticable, as nearly all net fishing in the vicinity of Corpus Christi had been stopped by legal restrictions that closed various waters to net fishermen.

The Texas game, fish, and oyster commission is cooperating admirably with us and has furnished, for the field work, a boat and fishing crew to operate our collecting gear, which includes various types of seines, trawls, and townets. All work up to the present date has been confined to the vicinity of Corpus Christi, a region always considered one of the best commercial fishing areas along the Gulf coast.

Most progress has been made in understanding the life history of the redfish, *Sciænops ocellatus*. Spawning has been discovered as taking place in the Gulf of Mexico in the late fall. The young have been secured in incredible numbers in the Gulf Passes, on their way to inside waters. The young redfish, on reaching inside waters, grow at a remarkable rate and when a year old have attained an average total length of 35 centimeters and a weight of more than 1½ pounds. They enter the commercial catch when they reach this size and probably are most valuable to the trade, although it is believed that the fish does not commence to spawn until its fourth or fifth year.

The second fish of commercial importance in Texas, and also an important fish of the Atlantic coast, is the spotted trout, *Cynoscion nebulosus*. This species has a spawning season that extends from March to September, with the heaviest spawning probably taking place in April and May. Ripe fish have been secured throughout the inside bays, and the taking of larval and young trout from 9 millimeters in length up seems to indicate that the trout spawn in the inside waters, although heavy spawning may take place in the vicinity of the passes that lead to the open Gulf. Larval trout never have been secured in Gulf waters.

The third fish to receive special attention is the black drum, *Pogonias cromis*. This is the most abundant food fish in Texas. Collections of young from 7 millimeters up together with the occurrence of spawning fish in the inside shallow bays, indicate that spawning takes place here. The spawning season seems to be from February to May, although fish have been found in a ripe condition in August. Spawning occurs as early as the second year.

The preceding bits of information will give you an idea of what the bureau is striving for in Texas waters. Our plans have been extended recently to include a systematic study of all fishes along the coast of Texas, with Mr. Ginsburg in charge of this work.

A plain-spoken Texas philosopher once said to me: "There ain't no fish here, they're all conserved." What did he actually mean? Did he mean that the fisheries of Texas actually are disappearing because of man's unselfish desire to conserve, or rather, preserve the unborn fish for his unborn great-grandchildren? No; the man was honest, and he really suggested the evil fact that all conservation might not be in good faith. That conservation was merely a cloak for personal desires to be satisfied. With the alarming decrease in Texas marine fisheries in spite of virtual prohibition of all forms of modern fishing gear, does not the suggestion of that old philosopher bring to your minds a new idea? Might it not be true that conservation of natural resources is exceedingly profitable for the present generation? Certainly the tourists to Yellowstone Park bring more to the State of Wyoming than the sale of lumber from the park ever would. Does it not pay to conserve these trees? Do not the tourists to Niagara Falls bring more wealth to that section than the com-

mercial utilization of the water power would? Hence the movement to protect Niagara Falls against utilization by electric-power concerns.

In Texas we have a powerful sportsmen's organization, whose self-appointed duty is the guardianship of the remains of the fish life. They have decided that every Texas citizen should have the privilege of visiting coastal waters and of fishing therein. But there is no fun in fishing when you can't catch fish. There is only one thing to do, and that is to conserve the poor fish by preventing their being caught in wholesale quantities. It is easy just to close all suitable water against commercial fishermen, and this, gentlemen, has been done in Texas.

We have an interesting economic problem before us. Would it not be of far greater economic value to Texas to encourage these thousands of sportsmen to follow a pastime that leads to the building of pleasure resorts, to the creation of many new occupations, and to the great advancement in real estate values rather than to allow a relatively small commercial fishery to interfere, at least in a psychological way, with the highly profitable trade of "soaking suckers?" That is a question that should interest some of you. Mr. Holmes suggested to me yesterday that a similar situation exists in Oregon in regard to the razor clam. The economic value of nonprofessional clam diggers in the community is apparently greater than that of the clams themselves.

How are we to distinguish true conservation from the vast amount of propaganda that pours forth from professional conservationists? You know they exist. They have demoralized the marine fisheries of the largest State in the Union; and, I understand, have caused trouble elsewhere. From a purely present-day economic standpoint they may be correct in their ideas. It is necessary that thought and attention be given to the problem.

Mr. Holmes. It might be well for me to qualify the example in Oregon that Mr. Pearson mentioned. I think the conditions there are somewhat different than those he related in Texas. There probably is more justification for the experiment and the tourists' attitude in trying to conserve the razor clam for their use; it could not support a very large industry, whereas it does bring in a very large number of tourists from every part of the country.

Mr. Sette. I would like to know whether the expansion of the commercial fisheries in Texas is really incompatible with equal enjoyment of the sportsmen's privileges in those waters. I do not see how it necessarily follows that you need to stop commercial fishing entirely if restrictions are sufficient to perpetuate the stock of fish. It should be possible to urge some reasonable degree of regulation, which would permit both factions to enjoy their occupation or sport.

Mr. Pearson. I pointed out that the sportsmen were not alone responsible for the decadent state of commercial fishing. There is a class of "poor" fishermen, who can afford only a few hooks, who at times make from $30 to $50 a day with hook and line, and they also oppose net fishing.

Mr. Radcliffe. I should like to make a few comments on the South Atlantic investigations. I think you are all impressed with the great diversity of the work in which the bureau is concerned in this region, including terrapin farming and stocking with fish to destroy mosquito larvæ.

I do not know whether it was modesty on the part of our chairman in not calling for comment on his paper on the mullet. I want to call attention to one point: In Doctor Bigelow's region, when you speak of fish you mean cod; when you come down to the South Atlantic and the Gulf, you mean mullet. The mullet is the important fish of the South. If it ever is marketed extensively outside of that area and others learn what a good fish it is, there won't be any left

for the southerners to consume. The mullet fishery represents a highly important field for scientific investigation.

Doctor Galtsoff. I wish to take this opportunity to point out briefly the extreme scientific interest in the study of the Gulf of Mexico. What I am going to mention may not be a job for the Bureau of Fisheries directly, because the relatively small importance of the fisheries there wouldn't justify prolonged scientific investigations, but I wish to call attention to the interesting oceanography in the Gulf of Mexico and its unexplored area. There is virtually nothing written on the Gulf of Mexico, except by Agassiz in his report of the Blake expedition. I searched the literature and couldn't find anything on plankton organisms, bottom forms, etc. At present, probably all shore fisheries are confined to the inshore products because of the currents outside of the lagoons, which make a boundary between the inshore waters and the Gulf itself. Because of the velocity of the currents the fishing is rather difficult. This should attract the attention of the oceanographer; the origin of the Gulf Stream lies in the Gulf of Mexico.

Mr. Pearson. In my talk I merely wanted to point out how ineffectual any scientific work would be in a State like Texas if public opinion is not converted to believe in real conservation. This is illustrated by the results of work we did in North Carolina, where we recommended a closed season. The commission first voted to have a two months' closed season put in operation at once. Then the fishermen got together, the commission got scared, and there were no regulations; so the work practically has gone to waste.

Mr. Thompson. In connection with the fact that conservation has run wild in Texas, it might be interesting to say something about the fisheries of California and the scientific work there. I believe Mr. Scofield will agree that the scientific work upon the fisheries has as greatly restrained unwise conservation as it has helped wise conservation.

Mr. Radcliffe. What is the length of the coast line of Texas, roughly?

Mr. Pearson. About 400 miles. The catch of food fishes is about 5,000,000 pounds.

Mr. Radcliffe. They must have a very poor fish supply out there, or else their commercial operations are having a very serious effect upon the supply that is there.

Mr. Higgins. The question of conservation in Texas is a little more serious than in some of the other States, because they have no offshore fishery. I wouldn't consider the snapper a Texas fish, although they do land some snappers. They depend upon three species almost entirely. No fishery can develop greatly if dependent upon such a small number of species; and these are shore fish, easily taken, and therefore subject more readily to depletion than are the pelagic species. I think the situation in Texas is not as hopeless as Mr. Pearson suggested, however. So far as the illustration that Mr. Pearson gave goes, the investigation in North Carolina showed a tremendous loss of immature fish. It is true that the fishery commission did not feel that they had public sentiment behind them so strongly that they could actually force a closed season. Yet the sentiment of the board was in favor of restriction, and they actually did pass some restriction on the fishing season. They have now about

10 days' protection, which is about one-fifth as much as they really need. I think sentiment will develop, and the work we do, whether it results in immediate regulation or not, will be appreciated later.

SCALLOPS

By J. S. Gutsell

The present scallop investigations, carried on chiefly at the Beaufort laboratory, were begun in the summer of 1925 at the request of the Fisheries Commission Board of North Carolina. The request was prompted by the great depletion of this valuable fishery.

The bay scallop fishery occupies an important place among the fishery industries of the State and is an extremely important one among those of Carteret County, to which it is confined. Of recent years it has approximated, in financial importance, the oyster industry of the entire State, with an annual value of nearly $250,000—an important item in the prosperity of the community.

The bay scallop had been studied previously by various investigators, particularly by Ritter, in Rhode Island, and Belding, in Massachusetts, with somewhat conflicting results as to growth and length of life. Belding's studies appear more convincing and his ideas have received wide acceptance. According to him, the life of the scallop is somewhat as follows: Spawning in early summer, growth to a considerable size before the first winter, the formation of a distinct winter ring, growth during the second summer to marketable size, and death before the next summer. Belding also studied spawning and embryology. He obtained fertilization by induced spawning but did not have much success with artificial fertilization nor succeed in rearing beyond a very early shell stage. The embryology seems to have been worked out well by him. He found no evidence of scallop migration. Drew has studied the sea scallop and various European workers the European forms. Of special interest are Dakin's memoir and Von Uexkull's study on Tonus.

I began these scallop studies with a general acceptance of Belding's views, but with a mind open for differences due to the different conditions prevailing (particularly climatic differences), and with the hope of extending our understanding and particularly of finding facts of immediate application to the local scallop fishery.

During the summer of 1924, a survey by the Fisheries Commission Board revealed an unusually great and widespread abundance of scallops. A later survey, following unprecedentedly heavy rains, showed almost complete mortality, except over unaffected areas in lower Bogue Sound. The scallop fishery, active from December 1 to 15 and from January 1 to April 15, confirmed these findings revealing scallops in great abundance, where found at all. Although direct evidence is lacking, there seems to be no reason to doubt that water-freshening from abnormal rains over the watersheds caused the mortality.

The first step in the investigation was to look for scallops. Dredging surveys revealed scallops in appreciable but not great numbers in one small area only, and that far from the laboratory. The area of recent abundance was extremely depleted. Fortunately, careful raking in the dense aquatic vegetation of the flats at the laboratory revealed scallops in considerable abundance. These flats then became and have remained the principal work ground. Surveys have been made from time to time over formerly productive areas. Recently it was determined to try, in addition, an area chosen because of the small size attained by the scallops.

The scallop is a hermaphroditic lamellibranch, possessed of the power to swim (for a short distance at a time), which inhabits shallow, grassy areas in decidedly salty water. Points chosen for special study were spawning, early life history and distribution, rate of growth, and age attained. Spawning is being studied by observation of gonadal conditions (both gross and histological) and by attempts at induced spawning and artificial fertilization. It has been learned that spawning occurs at least from spring to early winter. It has not been learned what are the governing factors, or definitely if there is a predominating or principal spawning time. Indirect evidence seems to point to the fall as the principal time, but this may be due to incompleteness. This is of possible importance for regulation of the open season. Attempts at artificial fertilization have failed in all cases to yield developing eggs. Attemps at in-

duced spawnings may be considered to have been unsuccessful, except on three occasions. On two of these development proceeded only to few cell stages. On the third attempt embryos developed well and large numbers of shelled larvæ were obtained. These grew beyond the latest stage studied by Belding, but not beyond the straight-hinge stage. They were measured, sketched, and supposedly preserved, but, like the extensive but incomplete embryological material, came through in very poor condition. It was thought that these and the prodissoconch shell on small postlarvæ would make the recognition of larval scallops a reasonably simple matter. However, very few straight-hinge larvæ like these have been found, and no larger forms that could be surely connected with them. Until recently no postlarvæ have been found that showed the prodissoconch shell at all clearly. Little progress, therefore, has been made in this direction. However, perhaps it is worthy of note that the larval stage is thought to be of brief duration, for the egg is decidedly larger than that of *Ostrea virginica* and the larva attains a much smaller size, only (about 0.18 millimeters in extreme length).

Growth rate and age are very important, from a practical standpoint, and are not so elusive as spawning and some other problems that confront the investigator. They have received special attention. Data compared have been entirely from Pivers Island scallops. At first, collections of 50 were made once a week. Later this was changed to 100 twice a month—a more satisfactory arrangement. Data thus secured are regarded as quite satisfactory, except at the two ends of the series of collections.

In January, 1926, at my recommendation, the scallop season was opened. Contrary to my recommendation, no exemption was made of flats at Pivers Island (the laboratory) and in the last three days of the season they were denuded almost completely of scallops, thus ruining all chances of obtaining satisfactory disappearance data. Data for small stages also were deficient. For quite a while no small scallops were seen; thereafter, until recently, only scattering numbers were found (except that a moderate size was attained in the spring of 1926). Yet the crop this year is comparable with that of last year.

Various methods of collecting material were tried. Fine nets were dragged through the grass, grass was collected and rinsed in water, and the water was screened—no scallops. After long searching, some were found attached to grass. Next, spreading and drying the grass on newspapers was tried with considerable success, but still with uncertain results. This fall, in desperation it was determined to rake up much larger quantities of "grass," a washtub full being chosen. Although this represents a greater amount of labor in the collecting than would be imagined, and some day's work in subsequent study, it has been adhered to with increasingly satisfactory results. I feel that if the Pivers Island flats do not fail me, satisfactory growth (and death) data may be expected by another year, and with them a good understanding of the spawning season. Of course, unless there is a great deal of improvement on the depleted areas, the data inevitably will suffer from extreme localization. Tagging experiments to obtain information as to growth, longevity, and spawning time are being tried on a small scale.

The growth data taken for mid-month periods are presented in a series of graphs (fig. 18), which, with sufficient clearness, show two year classes and indicate growth to market size in not much over a year. A normal fall growing period is indicated by the special graph for 1925 and by data for 1926.

Doctor Gilbert. What is the extreme age of the scallop?

Mr. Gutsell. I don't know. Belding's idea was about 2 years. He found a few spawning a second time. He found very plain year marks on the shell. Presumably because in the South there is winter growth, I didn't find anything that I could be sure of in the way of rings at this stage. Sometimes there would be three or four rings on a small scallop, one just as plain as the other. For a period of about three months (September to November) there is practically no growth. That is based on averages. This was also very plain by inspection of new growth in the fall. This year it was somewhat the same, but not so definite. What the explanation for the lack of growth in the fall is, I have no idea. It doesn't seem to have anything to do with spawning or anything else.

PACIFIC COAST FISHERIES

GENERAL REVIEW—THE INTERNATIONAL PACIFIC SALMON INVESTIGATION FEDERATION

By Dr. Willis H. Rich

As a very brief introduction to the outline of the salmon investigations, it might be well to give you a little idea of what we have in the way of salmon on the west coast.

There are five species, ranging widely from the coast of southern California up into the Arctic Ocean. The most valuable species, from the commercial

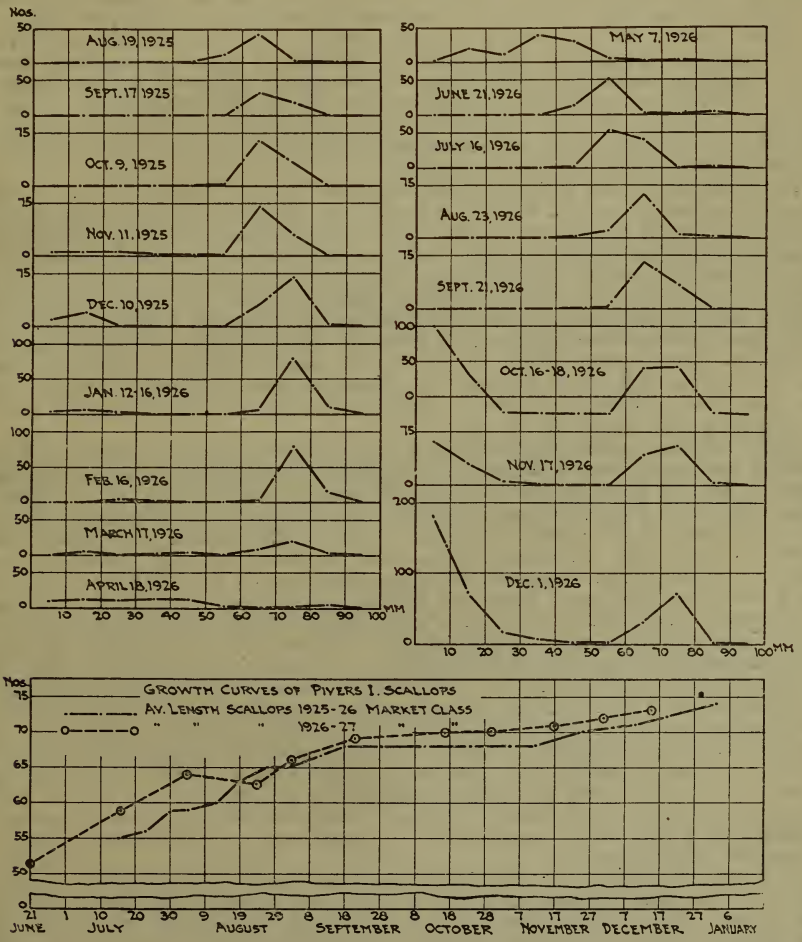

FIG. 18.—Length frequency curves (above) and growth curves (below) of scallops from Piver's Island, N. C.

standpoint, is the red salmon, and we have also the chinook salmon, the silver salmon, the chum, and the pink. In addition to these species of real Pacific salmon, there is a large, anadromous trout, or salmon, of the genus Salmo, closely related to the Atlantic salmon and with similar habits. This is known as the "steelhead trout." These fish are all anadromous, spawning in fresh water, the young going out to the ocean, remaining there for varying lengths of time, and returning to fresh water to spawn.

The habits, in so far as the life in fresh water and the life in the ocean are concerned, are quite different in the various species. The simplest type of life history is that of the pink salmon. The fish spawn in fresh water, the young go out to the ocean as soon as they emerge from the gravel and remain out there until they are 2 years old, when they return to fresh water to spawn. We have an invariable habit here—the fish always being 2 years old and having spent the greater portion of that time in the ocean.

The chum has, perhaps, the next most complicated life history. It has the same habit as the pink in going down to the ocean as soon as it emerges from the gravel, but it does not invariably return at 2 years of age, but at anywhere from 2 to 5, or possibly 6 years.

The silver salmon presents a still different variation. It remains, almost invariably, in fresh water for one year and then goes out to the ocean. It remains there for two years, returning as a 3-year-old fish. There are some variations from that, but the silver salmon, on the whole, is a 3-year fish.

There remain the red salmon and the chinook. The red salmon, with very few exceptions, remains in fresh water for at least 1 year but anywhere from 1 to 4, and then migrates to the ocean. It remains in the ocean for from 1 to 3 or 4 years and then returns. It varies in age, at the time of its return from 3 years (or possibly 2) to 7 years. The predominating age varies in different streams. In the Fraser River the predominating age is 4 years. In the Karluk River the predominating age is 5 years.

The chinook salmon presents other complications and ranges in age, at the time of maturity, from 2 to 7 or 8 years, the same as the red salmon, but, especially toward the southern portion of its range, has a marked tendency to migrate some time during its first year. It may migrate seaward immediately after emerging from the gravel, or, on the other hand, it may remain in until it is 1 year old or more. Spawning takes place in the fall. Many of the salmon do not migrate until the second spring following spawning, when they are approximately 18 months old, counting from the time the eggs were laid. In the Columbia River, particularly, which is the most important chinook-salmon stream on the west coast, the time of seaward migration is very variable and one may find seaward migrants in the lower portion of the Columbia River in almost every month of the year.

There are two well-known features of the life history of the salmon that have molded our conception of the need of conservation measures to a very marked extent. One of these is the fact that the salmon invariably die after spawning. That is a matter beyond question. The second, and most important, is the fact that the fish almost invariably return to the stream from which they came as little fish. In other words, what is known as the "parent-stream theory" is more than a theory; it is unquestionably a fair statement of fact. As a result of this habit, there has become established in all of the streams, and even in the tributaries of the larger streams of the coast, races of salmon. These may not always be distinguishable by physical characteristics (in fact, they resemble one another in everything, so far as any structural peculiarities or general habits are concerned), but we have every reason to believe that even in those cases where there is no possibility of distinguishing there are races, or incipient races, or something of that sort—self-perpetuating colonies of salmon in all of the separate streams and presumably in all of the main tributaries. We have considerable experimental evidence to support this, resulting from an extensive series of marking experiments and the evidence derived from scale examinations; and there is evidence, which has never been very carefully sought but which could undoubtedly be worked out in considerable detail, of structural differences.

The present problems of the salmon fishery are rather different from those of many of the other fisheries of which we have heard during the past two days. We know fairly well the general features of the life history of the salmon. We have methods whereby we can get any particular detail in the life history of any of the races or groups of races of salmon, if we want it—such as the age at maturity, the migration routes, and things of that sort. We have many data and methods well worked out whereby we can secure any particular item that we may happen to need. Furthermore, we have fairly reliable statistical records of the catches over rather long periods. In some places the statistical records are not as reliable as they should be; but in other places, particularly in some of the more important salmon-spawning areas in Alaska, the records are fairly good, and we feel that we are now in a position where we can begin

to make a study of the causes of fluctuations in abundance. We can now undertake not merely a study of the fluctuations in abundance, determining their existence, extent, and nature, but an investigation of some of the causes of these fluctuations, and a considerable part of our present program is directed along that line, although not exclusively.

A couple of years ago the United States and Canada and the fish commissions of the west coast organized a group known as the International Pacific Salmon Investigation Federation, for the purpose of coordinating the work of the salmon investigations. The State of California, the Canadian authorities, and the Bureau of Fisheries were each working on salmon investigations. This federation was for the purpose of coordinating these various efforts.

At the last meeting of this federation, a program was presented and was accepted by the executive committee of the federation which will give you, very briefly, about as good an outline as I can present of the work that is being undertaken and projected. With your permission, I shall simply read this report.

The subcommittee wishes first to express its belief that the primary purpose of any program of research should be to produce the essential knowledge needed for the proper and scientific administration of the salmon fisheries. Our desire is to conserve effectively the great salmon resources of the North Pacific, and our conception of such conservation involves the utilization of the resources to the fullest extent compatible with their perpetuation. We would like to be able to say definitely how many salmon it would be possible to take from a given region and still leave sufficient for spawning purposes so that the supply will continue year after year at a high level. It has been brought out repeatedly at both of the previous meetings of this committee, that the central idea about which we should build our program is the production of the maximum yield obtainable from this fishery; and by the maximum yield is meant the greatest production of fish that may be taken for commercial purposes without affecting the future supply. To provide adequately for this we must know: (1) What natural fluctuations in the abundance of salmon occur; (2) the causes of these fluctuations, particularly the immediate causes, though the ultimate causes should finally be known; (3) the intensity with which the commercial fishery is conducted and its effect on the future supply; and (4) the relative value of various measures that may be used to prevent depletion and to build up runs already depleted. With these fundamental requirements in mind, the following program is presented:

COLLECTION OF ADEQUATE AND UNIFORM STATISTICS

These are of fundamental importance and should include not only data that will show the trend of the yield of each stream or fishing area, but the trend of the fishing effort. Reliable statistics are indispensable to a determination of the fluctuations in abundance, and without these all our other efforts can be of little value. It is recommended that a separate subcommittee be appointed to consider the reliability of the present statistics and to suggest desired changes, either in the content, manner of collection, or manner of presentation.

TAGGING EXPERIMENTS

The tagging experiments conducted in the past have been productive of a great deal of valuable information relative to the oceanic migrations of the salmon and bearing on the relationship existing between the salmon found in various regions. This should be continued until we understand, with considerable thoroughness, these features of the distribution of the salmon along the coast. Such data are essential to the proper interpretation of the statistics, and only in this way can be know the true yield of a given stream or region. (The importance of such data is well known in the results obtained from the tagging along the Alaska Peninsula, through which it was determined that the fish caught in the Ikatan–Shumagin Island district were largely from the productive streams of Bristol Bay.)

SCALE ANALYSES OF THE ADULT RUNS

This is essential to any understanding of the causes, either immediate or ultimate, responsible for the fluctuations in abundance, and likewise will provide the necessary data on which may be based prophecies of future runs. By this means only can we determine the productivity of each brood year, the relative effects of heavy and light seedings of the spawning beds, the effect of various methods of artificial propagation, and of modifications in the intensity of fishing, etc. Such studies are being carried out already on a number of the more important salmon streams, and it is important that they be extended to others. They must be continued indefinitely as long as any salmon research is being conducted.

STUDY OF THE ADULT RETURNS FROM KNOWN ESCAPEMENTS TO THE SPAWNING GROUNDS

This line of investigation must be carried out on specially selected streams where the production can be accurately determined. The number of fish taken each year in the commercial fishery must be known, as well as the number of fish ascending to the spawning grounds. In this way the effect of variations in the spawning escapement may be worked out, as well as the natural fluctuations in production, from a given escapement.

Such studies are now being conducted on the Karluk and Chignik Rivers and at Alitak in Alaska, and at Cultus Lake in British Columbia. These must be continued at least until the more fundamental factors affecting the fluctuations in production have been determined. It will be impossible to conduct such studies on all salmon streams, especially on such large rivers as the Fraser and the Columbia; but it is believed that important principles may be discovered by the intensive studies on these selected streams, which will be of universal application.

STREAM SURVEYS OF THE SPAWNING GROUNDS

These, as conducted on the Fraser, Rivers Inlet, Skeena, and the Nass Rivers by Mr. Babcock, and in the Bristol Bay region by Mr. Winn, and others, are valuable as a substitute for escapement counts, where these last are impossible. They provide a fairly satisfactory measure of the escapement and the extent to which the spawning grounds are seeded.

STUDY OF THE PRODUCTION OF SEAWARD MIGRANTS FROM KNOWN ESCAPEMENTS OF PARENT FISH

This is being done by actual counting at Cultus Lake, in British Columbia, and the Bureau of Fisheries is attempting to get some measure of the number of seaward migrants at Karluk through marking experiments. It is important as a means for determining whether the greatest fluctuations in mortality occur during the fresh-water life or in the ocean, and will also serve as a measure of the results of conservation measures (such as artificial propagation) applied during the life in fresh water.

EFFICIENGY OF VARIOUS METHODS OF ARTIFICIAL PROPAGATION, AS COMPARED WITH NATURAL PROPAGATION

This line of investigation is one of the most practical and is designed to improve the present methods of fish culture. Intensive experiments are being conducted by the Biological Board of Canada, and extensive marking experiments have been conducted by the Bureau of Fisheries and the States on the Columbia, Klamath, and Sacramento Rivers. Not the least important phase of this work is the study of the differences in the efficiency of natural propagation under varying conditions. Very few reliable data on this point are available, and a united effort should be made to discover the facts.

EFFECT OF TRANSPLANTATION

This is of very great practical importance on account of its bearing on the possibility of restoring depleted runs by artificial propagation. Experiments designed to test the effect of transplantation on the return of salmon to the tributaries of a large system are being conducted by the California Fish and Game Commission on the Klamath River. Some of the marking experiments conducted on the Columbia River will touch this problem, and others are planned in British Columbia.

IMPROVEMENT OF SPAWNING AREAS AND OVERCOMING OF OBSTACLES, NATURAL AND ARTIFICIAL, TO THE ASCENT OF SPAWNING SALMON AND TO THE DESCENT OF THE SEAWARD MIGRANTS

Under this heading by far the most important question is that pertaining to the possibility of providing efficient passageways for both adult and young salmon over high dams. It is imperative that a solution to this problem be reached in the near future, if possible, else the salmon runs in some of our more important streams will be destroyed, or at least reduced to a point where commercial fishing will be unprofitable. It is recommended that a subcommittee be appointed to consider the advisability of making a complete study of this whole question.

THE LIFE HISTORY IN FRESH WATER, WITH PARTICULAR ATTENTION TO THE FACTORS AFFECTING SURVIVAL DURING THIS PERIOD OF THE SALMON'S LIFE

If we are to control, to any extent, the survival of young salmon in fresh water, it is essential that we know the causes of the fluctuations in the rate of mortality. It is important that we know the optimum conditions for the development of the eggs and young and the effect of variations in weather, water stages, temperature, chemical composition of the water, abundance of food, abundance of enemies, etc. This includes, necessarily, a study of the rate of development of the eggs, the growth and habits of the young, and allied problems. In addition to their bearing on the problems of natural propagation these questions also have to do with the problems of fish culture, and in that respect are of immediate practical value.

LIFE HISTORY IN THE OCEAN

Although many of these problems will be difficult of solution and must be deferred indefinitely, it is important, for the sake of completeness, that they be included in the program. The questions of the optimum conditions for survival in the ocean, habits, food, enemies, distribution, factors affecting migrations, factors affecting the onset of sexual maturity, etc., are all important to a thorough understanding of the fluctuations in mortality rates during the life in the ocean; but since they are not, apparently, subject to any control by man, these problems do not appear to this committee to be of prime practical importance.

STUDY OF THE EFFECT OF SEA FISHING

It is generally believed that trolling and purse seining for salmon in the ocean, as at present conducted, is destructive, especially of immature fish. Various details necessary for intelligent control of this fishery should be worked out in each locality. From the tagging of troll-caught fish we will ultimately learn the interrelations of the fish caught at different places along the coast; and we should know, in addition, certain details for each fishing locality, such as the percentage of immature fish taken at different times during the year, the loss due to fish hooked but not landed, etc.

It is recognized that many of the fundamental facts of the life history of the salmon are fairly well known; in this respect we are fortunate in having a good foundation on which to build. We have good methods for tagging, marking the young fish, and for making scale readings, and these may be applied to the working out of many of the details that will be required. Many minor details can be and are being studied in connection with some of the larger problems mentioned, but it has not been thought necessary or desirable to bring forward at this time too many of the details.

Not all of the items in this program can be undertaken immediately, but it is hoped that ultimately the program, in its entirety, may be accomplished. We wish to emphasize, however, the importance of selecting the more important practical problems for first attention. These practical problems may not be the most interesting or the most fundamental, but may be urgently required as a basis for immediate administrative action, without which serious and perhaps irreparable damage to the salmon runs may results.

We have been conducting special studies on the salmon stock in Bristol Bay, Alaska. Bristol Bay is the greatest red-salmon producing area of its size in the world. There are a number of large streams, each heading in large lakes, one (Iliamna) being nearly 100 miles long. There are other lakes above Iliamna, connected with it by rivers. Spawning of the red salmon almost invariably takes place in these lakes, where the young remain for a year or two. At one time there were 30 or 40 canneries in this region. The production is upwards of 1,000,000 cases a year. The question was whether the fish taken in this fishery were of local origin or from somewhere else. Of the fish we took, we found that a considerable percentage belonged in Bristol Bay. That naturally threw a light upon the conservation problem there.

That is an outline of the general program. I, as you know, have been out of the work for some time and have only very recently gotten back into it, so I shall ask Doctor Gilbert to tell you more about the work that actually has been accomplished on the west coast recently and for which he mainly has been responsible. Doctor Gilbert, as you know, is the leader, I might almost say the originator, of fisheries research in this country, and I presume there are a great many of us here to-day who would not be here if it were not for the work Doctor Gilbert has done.

SALMON

By Dr. C. H. Gilbert

I shall speak briefly on the work we are attempting to accomplish in the Kodiak region, to which Doctor Rich has referred—a region that lies south of the Alaskan Peninsula and is the site of an important salmon fishery.

Among the multifarious duties that devolve on the Bureau of Fisheries there are no others, I venture to say, that are so exacting and entail such heavy responsibilities as the administration of the salmon fisheries of Alaska. In 1924, recognizing the precarious condition of these fisheries, Congress passed a bill that granted extraordinary powers to the Secretary of Commerce to enable him to control the fisheries of Alaska and prevent their further depletion. The powers thus granted made it possible to limit the fishing in any district to whatever extent seemed necessary, or to close it to fishing entirely for a term of years, if serious depletion already had taken place. The kind and extent of fishing could be prescribed in any area, and, in general, such regulations imposed as would prevent the disastrous effects of overfishing or would restore the yield in such districts as had suffered already. The entire responsibility for maintaining the yield of this most important salmon fishery was thus placed squarely on the Secretary of Commerce, and through him on the Bureau of Fisheries.

No one except those who have been directly concerned with the administration of these fisheries can appreciate the complexity of the problems and the inherent difficulties of the task. There are five different species of salmon of

commercial value in Alaska, each species presenting a series of problems distinct from all the others. Furthermore, each of the almost innumerable streams of Alaska has, for each of these five species, its individual colony of spawning fish, which must have independent protection. The fishery is based exclusively on the spawning run, on the fish which, at maturity, are seeking their native stream for purposes of propagation; and it is obvious that the future of each stream depends on there being provided, each year, an adequate spawning escapement from the membership of its own colony. Regulations must be devised and enforced, which will so hold the fishing in check that enough of the salmon bound for each stream may escape the nets, to serve as parents for a future generation. Failure to accomplish this for any year will make serious inroads on the salmon supply when the inadequate progeny of that year shall have matured and themselves comprise the spawning run of the year.

It must be remembered that when these administrative problems were imposed on the Bureau of Fisheries a very extensive fishery was already established. Millions of dollars were invested, many thousands of men depended on it for their livelihood, and the products of the fishery formed an important food supply for domestic consumption and in foreign commerce. This industry had developed in entire indifference to the future. With each year all the salmon were caught that could be caught to commercial advantage, with no concern as to the size of spawning reserves. The inevitable consequence had been partial depletion, in varying degree, throughout the coasts and streams of Alaska. The damage already done involved not only the current year. Salmon of the different species mature at different ages, from 2 to 7 years, and the progeny of the inadequate brood years of the past were already in existence and must form our sole dependence for salmon runs extending over a term of years.

Had it been feasible to abolish the industry out of hand, increased spawning of all species in every stream would have resulted immediately, and rapid improvement could have been predicted confidently. But nothing approaching this simple solution was possible. A gradual curtailment of fishery operations was all that could be hoped for, which would occasion as little harm as possible to the fishery and at the same time insure progressively larger spawning reserves. It was a new experiment in fisheries conservation, the exact results of which could not be foretold. Regulations to insure the desired result called for the establishment of a delicate balance between commercial catch and spawning reserve. The effect of the regulations must be made the subject of careful analysis in each district, and modifications made from year to year, as experience should dictate. The pressure of this responsibility has been so great that Commissioner O'Malley has felt constrained to take the field each year in person, and has found full scope for his intimate knowledge of the Alaska situation and his wide practical experience of the fisheries.

As Doctor Rich has pointed out, the scientific staff of the bureau fortunately had made substantial progress in working out the life histories of the salmon before these heavy responsibilities devolved upon the bureau. A firm basis for conservation measures was thus at hand. But with the assumption of new duties toward the salmon, the demand for increased knowledge was at once experienced, and some of the unexplored regions took on a sudden and pressing importance. Among these unsolved problems, and the most urgent of them, was the question of how large a spawning reserve could be considered adequate in any stream, or, stated in different form, how large a yield can normally be expected from any given number of spawning fish. No even approximate answer to this problem was at hand, and yet the bureau found itself suddenly confronted with the duty of providing an "adequate spawning reserve" in each of the streams of Alaska. It is to throw light on this all-important subject that the Karluk and Chignik experiments have been devised.

The urgent necessity for this investigation had been appreciated even prior to 1924, and a beginning had been made before that date. The difficulties of the problem were seen clearly. The normal or average yield from a given number of spawning salmon could be expected to vary more or less widely for the same species in different streams, and would surely vary extensively for different species even in the same stream. Furthermore, a certain amount of variation could be anticipated between one year and another for the same species in the same stream.

The final yield from a given number of spawning fish would depend on percentage of successful spawning, percentage of escape from destructive physical agencies, from diseases, and from predatory enemies; and these, with

other destructive factors, would vary with the species, with the stream, and with the season. It could confidently be expected that certain seasons, in a given stream, would be more favorable than others, and it might well happen that a smaller spawning reserve in a favorable year would produce a larger ultimate run than would be produced by a much larger spawning reserve in a year less propitious. These fluctuations, due to natural causes, are independent of the size of the spawning colony and form one of the greatest obstacles to a scientific administration of the fisheries. Through them, even the fact of depletion becomes difficult of detection, and the trend of the fishery can be determined only through observations extending over a considerable period of time. They make difficult, also, the solution of our special problem here considered, and will produce such a variation in our results, from year to year, as will yield us nothing more than an average expectation on which to base our procedure. Even this, however, will be invaluable. We shall have established what is a normal or average expectation of yield from a given number of spawning fish in the stream in question, and we shall have a basis for exact determination of the full swing of the pendulum of natural fluctuations in the yield of these colonies, a basis that heretofore has been lacking.

We selected as our species the red salmon of Alaska (*Onoorhynchus nerka*)—the most valuable species, commercially, and the one concerning the life history of which the most is known. It develops spawning runs in such streams only as have one or more available lakes in their course, and spawns almost exclusively along the gravelly shores of the lakes or in their tributary streams. The young, after hatching, seek the deeper waters of the lake and live there for varying periods before they enter on their downward migration to the sea.

In selecting streams for our experiments, it was necessary to find such as admit annually of a complete census of the run, including that portion that is taken commercially, as well as the portion that escapes the fishermen and ascends to the spawning grounds. Such streams are difficult to find, for usually at some portion of their spawning migration the mature fish mingle with those bound for other rivers and are there the subject of an important fishery. It is impossible to segregate the different races involved in such a fishery, and the commercial part of the yield is therefore left in doubt. Fortunately, in the Karluk River, on Kodiak Island, and in the Chignik River, on the southern slope of the Alaska Peninsula, we have two important red-salmon streams, the runs to which are substantially free from capture until they reach the vicinity of their respective mouths, where the fisheries are located. These two streams have been selected for our experiments. Agents of the bureau collect daily statistics of the commercial catch, and the spawning escapement is ascertained at counting weirs, one of which is operated in each of these rivers. The Karluk weir has been maintained yearly since 1921, and the Chignik weir since 1922.

Such a weir consists essentially of a picket fence extending entirely across the river, with a number of adjustable gates, through which the ascending salmon are permitted to pass, virtually in single file. Employees of the bureau are stationed at these gates day after day throughout the season and count the fish as they pass through. The run in the Karluk River begins in May and continues uninterruptedly into October. During this long period the watchmen are constantly on duty, shutting the gates during the darker hours of the night or for limited periods in the daytime when the run is slack; but otherwise, in wind and rain and the searching cold of the Alaska summer, they stand on the weir and count the salmon streaming through the gates. Although the run is constantly in progress during the summer months, the numbers that pass on different days varies widely, from a few hundred on some days to over 100,000 on others. The magnitude of the task becomes apparent when it is considered that in the season of 1926 over 2,500,000 red salmon were counted through the Karluk weir.

If all the progeny of a given year matured at the same age and returned in the spawning run of a single year, our problem would be a relatively simple one. Unfortunately, this is far from being the case. They may mature at any age from 3 to 7 years and return mingled in widely varying proportions with salmon of different age derived from other brood years. To ascertain the yield of any single year's spawning, therefore, it becomes necessary to analyze the runs of all the different years in which the progeny may appear. The first year the Karluk weir was in operation was 1921, when about 1,500,000 spawners ascended the river to the lake. Part of these returned as 3-year fish in 1924, as 4-year fish in 1925, and as 5-year fish in 1926. Others may be expected as 6-year fish in 1927 and as 7-year fish in 1928.

Completely to ascertain the yield from one year's spawning in the Karluk necessitates, therefore, the full analysis of the runs of five consecutive years, in each of which we ascertain the proportion of the fish of each age present in the run, and ther actual number. As the runs are not homogenous throughout the season, but vary from time to time in the proportion of fish of different age, it becomes necessary to take adequate samples at frequent intervals. An employee of the bureau who is stationed on the Karluk fishing grounds takes therefrom a sample of scales from 100 to 120 salmon on every available day during the fishing season. The number thus obtained for investigation during the season of 1926 was in the neighborhood of 9,000. Data of length and weight and sex are obtained for each fish, and from the scale analysis we learn the length of early residence in the lake and the age at maturity. Complete analyses thus have been secured of the runs of 1924, 1925, and 1926; and we have learned from these that while, as has been stated, the total range of age at maturity is from 3 years to 7, the great majority of the Karluk red salmon mature at the age of 5. The average of 5-year fish for the three years mentioned is approximately 80 per cent. Now, the 5-year fish from the 1921 spawning matured in 1926, so, although we do not as yet have complete returns from that spawning, we are now in possession of figures that make possible a fairly reliable estimate. This is of special interest as furnishing the first information we have ever had concerning the returns from a known number of salmon spawning under natural conditions.

The spawning reserve of the Karluk in 1921 consisted approximately of 1,500,000 salmon. The run in 1926, over 80 per cent of which was derived from the 1921 spawning, consisted of over 4,500,000 fish. The final figures will not vary widely from these and will thus show an increase of approximately 3 to 1. The significance of these figures we are not now in position to estimate. How wide the oscillation in productiveness is from year to year, due to natural fluctuations, we do not know. Neither can we tell whether the ratio of 3 to 1 lies nearer the maximum yield of the more favorable years or nearer the minimum, or whether it most nearly approximates the mean. Some light on this problem may be expected in 1927 and in succeeding years. As it happens, the spawning escapements to the Karluk varied widely during the first few years of the operation of the weir. With 1,500,000 in 1921, less than 500,000 in 1922, over 670,000 in 1923, and over 750,000 in 1924, we shall have presented to us a series of runs representing returns from spawning colonies of widely different sizes. If the 3 to 1 ratio observed as between 1926 and 1921 is substantially reaffirmed as between 1927 and 1922, the 1927 Karluk run will be of such small size as to yield little if any surplus to the fishery there located.

These disquieting facts have been communicated by the bureau by way of warning to the companies concerned, coupled with the qualifying statement that in the present early stage of our investigations our experience is too limited to serve as the basis for confident prediction. If the ratio of return in 1927 should prove much higher than in 1926, a satisfactory run might result, despite the very limited spawning of 1922. The result will be observed with the greatest interest. Whatever it may be, it will add materially to our present knowledge. With each successive Karluk year, further additions will be made. In 1927 we shall have similar returns from the Chignik, to be followed by an unbroken series also from that river. In the near future, therefore, we should have ascertained how close a correlation exists between the size of a spawning colony and the extent of the run that it produces.

Mr. SETTE. With reference to this 50 per cent escapement, isn't it reasonable to suppose that at times the natural run, due to natural causes, might be much below the usual number, and by allowing a 50 per cent escapement of this natural run you would have much less than the necessary amount to produce a normal run in subsequent years? If the normal run amounted to 5,000,000, and in an off year you got only 1,000,000, would not the fluctuations be continued by having merely a 500,000 escapement in that year, as compared with a normal escapement of 2,500,000 in a normal year? Would it not be possible, by having a fixed escapement for any certain river, to control to some extent the natural fluctuations?

Doctor GILBERT. That point is one that occurred to us at the beginning of our Karluk experiment. It must be apparent to you that on

some years we got not only one-half of the total run, but the entire run.

We proceeded to deal with that situation in a modest way. We did not know at the time what the total run of the Karluk River was. We did not know what the ordinary escapement was. We did set, in the case of the Karluk and Chignik Rivers, an arbitrary number. The number that we fixed at that time was 1,000,000. That is the minimum on the Karluk River to permit of a commercial fishery. The same thing is true on the Chignik. We may find later that the minimum that we have set is entirely too small to answer our needs.

In 1922, the second year of our operations on the Karluk River, before this provision had been adopted only 500,000 fish proceeded to the spawning grounds. We expect returns from those fish next year.

The following year, 1923, about 670,000 fish went to the spawning grounds.

Mr. Radcliffe. I would like to stress the tremendous economic importance of this work. The world production of solmon in 1924 was about 1,000,000,000 pounds. Of that, the United States produced 520,000,000 pounds and British Columbia 197,000,000 pounds; or, the combined production on our North Pacific coast was seven-tenths of the world production.

The production of salmon in the North Atlantic is very small—less than 1 per cent.

This tremendous salmon-canning industry started in the States. As the fishery became depleted, the canneries moved northward. They are at the outpost of that industry. Either the industry must be maintained where it now exists, or we shall have to do without salmon, unless, perhaps, we can look to the southern seas.

Doctor Gilbert mentioned the fact that the scientist in the North is on trial. I sometimes wonder if it wouldn't be a good thing if more of us were on trial.

Mr. Thompson. Doctor Gilbert is in somewhat the same position that we are in the case of the halibut. The North Pacific seems to have been chosen as a ground to try the biologist. The great problems he has met and his statement of them apply very aptly to the halibut. I think that as I gain in experience with the marine fisheries, it is impressed upon me more and more that the fundamental problems to be met are very much the same, and that as the biologist is brought to face the facts in other regions, he will have to come to the same conclusion. I think those statements of fundamentals in the salmon problem, stated perhaps a little bit differently, would apply to a great many other fisheries.

We have in the inshore fisheries quite a problem as to the relative rate of fertility of the young and older stages. That has become of considerable importance in such fish as herring and halibut. The salmon goes to sea in its second year and remains for 4, 5, or 6 years. There is an interlude of sea life, and there is no way of getting at the mortality then.

I am interested to observe that there is a definite correlation between the spawning runs in the case of the Fraser River and the returns to that river. The fluctuations in the sea have not been so great nor so marked as to destroy that correlation between the spawning run and

spawning return. During the sea life of the salmon the fluctuations in mortality are not sufficient (in the case, at least, of the Fraser River) to destroy the correlation.

That last statement regarding the general correlation and the fact that there is very little destruction in the sea, is another interesting aspect. If a very small number of fish returned in any particular year there would be two possibilities—either that they had not been successful in their early natural history, or else that there had been some destruction out in the open sea. That makes possible, I should think, a very interesting study of the needs of the young fish.

Doctor GILBERT. As regards this matter of destruction in the sea, we must not minimize the amount that occurs. There is always a very great destruction. The best evidence we have comes from our experiments with marked fish. Doctor Rich has been foremost in this investigation, and for many years we have marked large numbers of fish so that they could be recognized on their return, and the ratio of return to the number marked (assuming that the marking has not caused any injurious effect on the young fish) will give a fair evidence of the destruction in the sea.

Doctor RICH. I think the work of marking will be handled by Mr. Holmes. He has been taking charge of that work on the Columbia River, and I think he has that all in hand.

Doctor GILBERT. With reference to the amount of destruction that occurs to fish on entering the sea from the Karluk River, we believe we have some very favorable results in that respect. The majority of fish remain until their third year, when they proceed to sea. They are somewhere between 5 and 8 inches long, and we believe are fairly well able to take care of themselves. At the same time, I shall be very agreeably surprised if we find, on the completion of our investigation in the Karluk River (which Doctor Rich now has in progress, he having marked some 47,000 fish on their way to sea last year), we have a return of more than 10 or 15 per cent of these fish. So that destruction in the sea is not negligible, and the fact that it has not, in all cases, succeeded in destroying the correlation between the spawning and the final returns, merely shows that there are fluctuations in spawning which may not easily be overcome by destruction in the sea.

I may emphasize, at this point, our experience in the Fraser River. In this river we had had, from time immemorial, a very extensive run every fourth year and small runs in the intervening three years. In the fourth year there was very heavy spawning, for some unknown reason. Far back, beyond historical times, the four-year cycle became a fixed feature in the fishery of that stream, but by a very unfortunate occurrence in one of those fourth years the run was checked. The fish were not permitted to reach their spawning grounds, and the peak run in the fourth year was lost entirely and never was recovered.

I must call your attention to the fact that the pink salmon, or the humpbacked salmon, is a 2-year fish, as Doctor Rich has said, and we have alternate cycles almost universally throughout the range of the pink salmon. Every other year is a successful year. There is no overlapping of groups. We do not have 3, 4, 5, 6, or 7 year fish to help breach over the results of unsuccessful spawnings; so

when an unsuccessful spawning occurs, two years thereafter there can not be a large run. Sometimes when a successful spawning occurs, unfortunately, a large run does not occur two years thereafter. It is possible to undo the effects of a large spawning and not possible to overdo the effects of a small spawning.

Mr. THOMPSON. Is it your opinion that the large mortality in migrating to sea has in very many cases destroyed those 2-year and 4-year cycles? Are these cycles not evidence, rather, of the stability of that correlation?

Doctor GILBERT. So far as that is concerned, I am ignorant at this time. The correlation will be between the commercial catch of a given year and the returns 4, 5, or 6 years thereafter. The correlation will have to be between the spawning escapement and the adult return. Unfortunately, prior to the present era, we have no statistics except those for the commercial catch. It may very well be in a given region that the commercial catch has a definite relation to the spawning escapement. In Bristol Bay the fishing is conducted exclusively by gill nets operated from small boats. If the season is very stormy the small boats can not operate freely and we will have a very much larger percentage of fish escaping than we have in a season of good weather in which it will be possible to fish every day. You can see that our correlations are based on very inadequate data.

Doctor DAVIS. In regard to the marking experiments, we thought it would be very desirable to mark the young trout last summer. We cut off the fins, etc., and in practically every case within two months they had almost grown out again.

Doctor GILBERT. If you had consulted with Doctor Rich before you attempted that experiment I think you might have had better results. We had this same experience. The known power of regeneration of fins was a difficult factor to deal with until we ascertained that if you cut deep enough below the articulation of the rays with the underlying structures no regeneration will occur. You may cut the caudal fin off one-half inch from the base and that entire fin will regenerate. I took a harness punch and made circular cuts in the fin. They also filled up completely. There was nothing to show that this had been done except some pigment markings. If you will cut deep enough, you will find that no regeneration will occur.

Mr. BOWER. I have had something to do, more particularly in an administrative way, with the Alaska fisheries for more than 15 years, and I think I am in position to pay tribute to the excellent results that have been gained by Doctor Gilbert and Doctor Rich and others in solving the problems that confronted us when we sought to protect and conserve the salmon fisheries in Alaska. I can not use too high terms in indorsing the work that has been accomplished.

Perhaps the record should be made to show the extent of the fisheries of Alaska commercially. Employment is given at present to approximately 25,000 men. There is an investment of about $60,000,000, and the products, annually, are valued at substantially $40,000,000. That gives some idea of the extent of the industry with which we are dealing. Prior to the enactment of the law of 1924, to which Doctor Gilbert made reference, we dealt with the fisheries of Alaska as best we could under the law of 1906. We realized its inadequacies, but we used the limit of authority given

by it in conserving the fisheries of Alaska. Nevertheless, serious depletion occurred in many places. The law of 1924 gave us the authority we needed to say when, where, and how fishing might be practiced. It seems not inappropriate for me to mention one or two of the results, in a practical way, of that new law and the regulations promulgated by the Secretary of Commerce.

The outstanding practical achievement is the return of the pink salmon to Alaska waters in the season of 1926. I think the regulations of 1924 may be given full credit for this result, for the reason that the pink salmon is a 2-year fish, and the fish that escaped in 1924 by virtue of the department's regulations produced, in 1926, the largest run of pink salmon in the history of Alaska. The commercial catch was more than 1,000,000 cases in excess of the largest previous catch in the history of the Territory. That is a direct tribute to the scientific work that has been done, because the regulations are based chiefly upon the results of science.

The other thing I want to mention is that we have a commissioner of fisheries who has courage, foresight, and ability to deal with this situation. Doctor Gilbert spoke of the dilemna with which we are confronted. The commissioner knows only too well the troubles that visit us, from an administrative standpoint, in dealing with this problem. Sometimes our best friends are after us with the sharpest sticks. I want to pay the highest tribute to the commissioner of fisheries in his administration of these fisheries. It is an inspiration to work with him and for him in such an undertaking.

Doctor Rich. I wonder if we might not hear from Mr. O'Malley on some of the experience he has had recently in regulating the salmon fishery of Alaska.

Mr. O'Malley. Possibly Doctor Gilbert could tell it better than I can. Looking back on the catch and the escapement in Karluk in 1922, and comparing them with 1926, there seems to be a three to one ratio—three fish this year for every one that spawned then. At that time Doctor Gilbert called my attention to the fact that with such a small escapement it might be a good plan to close the fishery this coming year and let the fish have a chance to rehabilitate themselves. Doctor Gilbert also pointed out to me the small percentage of grilse noted in this past season, which might indicate that we need not expect a successful run in 1927. In fairness to the three companies operating there, we thought it best to close up shop, leave the run untouched, and accept the suggestions that came from the scientific investigators. However, there were local needs to be considered, for the natives are dependent for their livelihood upon work furnished by these three companies. Therefore, the department followed a more moderate course. It was thought better to warn the companies of an impending poor season and let them make their preparations accordingly, on a small scale. We have left the regulations as they were last year and notified the companies what we consider the run may be next year; we don't say positively it will be poor, but we have every reason to believe that it will be. As a result of this warning, I believe the companies will consolidate and operate on a small basis. If we are able to make predictions successfully regarding the size of runs it will save the industry millions of dollars. What we hope to do later on is to be able to forecast, with some degree of accuracy, what will happen in the future.

We have a problem confronting us in Bristol Bay, where there have been poor runs for several seasons. I think that if we can forecast with some accuracy what the next season's run will be, the people in Bristol Bay will be willing to forego a season's operations in order to take care of the run and give it an opportunity to rehabilitate itself. There is considerably more spirit of cooperation now than we had in the summer of 1924. I wondered then whether it was better to let the industry drift another year and make excuses to Congress or to take aggressive action.

I have just received the following telegram, which I shall read to you:

SOUTH BELLINGHAM, WASH.,
January 6, 1927.

HENRY O'MALLEY.
United States Commissioner of Fisheries,
Washington, D. C.

After going into records of Bering River very carefully we are convinced that in the interest of conservation of fish this section should get all escapement possible this season. It is always our desire to cooperate with the bureau in matters of this kind and it will be agreeable to us not to operate this plant during 1927, if you will issue an order closing the Bering River district. It is our sincere belief that this should be done, and we therefore believe it our duty to recommend it to you.

PACIFIC AMERICAN FISHERIES.

If we can get more people into that attitude, there is some hope for progress.

Doctor GILBERT. Since the question has arisen as to the amount of doubt in our prediction for the next year, I wish simply to state the nature of that doubt: What is the relation between variations in natural production, due to causes which we have not been able to identify, and the spawning reserve? We know what the spawning reserve was, the commercial run, we know in any year what a given spawning produced, but we have not been able to ascertain to what extent spawning reserves will vary in their productiveness. In other words, is it possible that a spawning reserve of one-third million may, under extraordinary conditions, produce a larger run of fish than is ordinarily expected?

Mr. BOWER. I want you to know that the splendid cooperation of the trade is due, in a large measure, to Mr. O'Malley's bringing diverse interests together. I am confident that the future of the salmon industry of Alaska is absolutely safe under the regulations of the Secretary of Commerce.

COLUMBIA RIVER SALMON

By H. B. HOLMES

As one of the most important means of studying the life histories of salmon, the Bureau of Fisheries, in cooperation with the Oregon Fish Commission, has conducted, during the past 10 years, an extensive series of marking experiments. In these experiments young, artificially reared salmon have been marked by removing certain of their fins and then have been liberated into the streams on which the various hatcheries are situated. The marked individuals have been identified by the absence of the fins when they were caught upon their return to fresh water to spawn. By means of these complete records of individual fish, our knowledge of the biology of the species and the success of various hatchery practices has been increased materially.

Twenty-six experiments, involving over 1,000,000 fingerlings, have been conducted. As a result of these markings, over 7,000 adult fish have been recov-

ered, and additional returns from some of the experiments are to be expected during future years.

The collection of the data from the returning adults has proved to be a difficult matter. Some representative of the Bureau of Fisheries has spent the greater part of each season in the commercial fishing district, but it has been necessary to depend, for the greater part, upon the assistance of fishermen, cannery employees, and hatchery men for these data. The majority of the returns have come as a result of the payment of rewards for records of recoveries.

Two species of salmon have been marked—the chinook salmon (*Oncorhynchus tschawytscha*) and the sockeye salmon (*Oncorhynchus nerka*). The experiments with sockeye salmon will be considered first.

A thorough understanding of the sockeye marking experiments will require a knowledge of the history of the Columbia River fishery for *Oncorhynchus nerka*, which is known there as "blueback salmon."[16] In the early years of the fishery this species contributed an important part of the pack. For the five years, 1890 to 1894, it supplied an average of 43,000 cases. With the exception of a large pack in 1898, there was a rapid decline until the low level of less than 6,000 cases was reached in 1907. For the five years, 1904 to 1908, the average pack was about 9,000 cases—less than one-fifth of that of the period 15 years earlier.

In 1905 the Oregon Fish Commission made an effort to rebuild the run by artificial propagation and built a hatchery for that purpose on the Wallowa River. For some unknown reason, no bluebacks were intercepted by the racks at that station. Having failed in this undertaking, and knowing of no other tributary in which spawning bluebacks might be found, the Oregon Fish Commission called upon outside sources for a stock of eggs. In 1910 they received a shipment of 1,500,000 sockeye eggs from Alaska. Similar shipments of Alaska sockeye eggs were obtained each following year.

In the years in which the imported sockeyes should have reached maturity, none entered the tributaries in which they had been liberated, and there was some question as to the success of their propagation. It was suggested by some that the fish had returned to the Columbia River but not to the particular tributaries in which they had been liberated, the supposition being that in this case they would have been mistaken for native bluebacks. Other persons, who were not convinced of the truth of the "parent stream" theory, suggested that the fish might have entered some other river system for spawning.

The marking experiments with sockeye salmon were undertaken primarily to determine these points. The first experiment consisted of a marking of 50,000 yearlings, which were liberated in a tributary of the Columbia River in March, 1916. Adult fish from this experiment returned to the Columbia in 1918 and 1919, when they were in their fourth and fifth years. Authentic records of about 100 of them were reported from the commercial fishery. This number is of little significance, however, as no rewards were offered at that time, and as a result the reported returns are known to have represented only a small part of the actual recoveries.

The most interesting fact coming from this experiment was that these Alaska sockeyes differ sufficiently from the native bluebacks as to make it possible to distinguish the two classes of fish from each other by appearance only. This suggested the question of why these fish had not been observed in the Columbia River during the preceding four years when results of the propagation of the sockeyes should have been obtained. The only logical answer to this question was that the propagation had not been successful. The confidence in this answer was strengthened when it was observed that the fingerlings in former years had been liberated at a much earlier age. More conclusive evidence was obtained by a second marking. In this experiment the marked fingerlings were liberated during the fall of their first year, instead of being held until the spring of their second year, as in the first experiment. Not one return was obtained from this marking. It is evident, then, that under the conditions that prevail at these Columbia River hatcheries, success with sockeyes obtains only when the fingerlings are held until the spring of their second year—until their normal time of seaward migration. These results might be presented as evidence of the necessity of carefully checking the success of hatchery operations. In this instance it has been shown that success does not necessarily result from the liberation of large and vigorous fingerlings.

[16] As a matter of convenience, the term "blueback" will be used in referring to the fish native to the Columbia River, and the term "sockeye" will be used in speaking of the imported fish. Both are of the species *Oncorhynchus nerka*.

The quantitative results of rearing sockeyes to the yearling stage have been shown best by three later experiments, in which rewards were paid for records of recoveries. The reported returns from these experiments represent 1.4, 2.5, and 4.8 per cent of the number liberated. The last two percentages represent only the 4-year-olds, which were recovered during the past season; additional returns from these experiments are to be expected in 1927.

The returns have been expressed in relation to the number of yearlings liberated, because the data of the mortality in the hatchery are not sufficiently accurate (in most cases, at least) to be of value. In one case, comparatively accurate records of the entire history of the fish are available. In round numbers, the history is as follows: Starting with 100,000 eggs, 50,000 yearlings remained at the time of marking in February of the second year; 2,400 adults were caught in the commercial fishery when they returned in their fourth year; and an additional 100 4-year-olds passed through the commercial fishery and returned to the hatchery at which they had been liberated. From this 100 fish, 150,000 eggs were taken. The cycle ends, then, with a stock of eggs one-half greater than the number with which it started. The total number of adults recovered represents 2½ per cent of the number of eggs required to produce them.

A return of 2½ per cent may, at first thought, seem very small, but the impression is quite different when the fact is taken into consideration that each female sockeye produces two or three thousand eggs. The return in this experiment was at least 30 fish to each parent fish. Hatchery operations that provide a return to the commercial fishery of 30 fish from each parent and a spawning escapement sufficient to double the stock in two generations must be considered as successful.

The artificial propagation concerned in this particular experiment has been underrated in several ways. The quoted returns represent only the 4-year-olds. The 5-year-olds will return next season. The spawners did not all return to the place of liberation; only those that did have been considered here. It must be pointed out, however, that this experiment has been more successful than any other in producing returns. Similar practices may or may not produce equal returns.

Possibly the most valuable contributions to be added to our knowledge by these experiments are in regard to the habits, instincts, and racial peculiarities of the fish. One of the racial peculiarities is that of storing fat in the flesh. Chemical analyses of canned salmon have shown that the blueback of the Columbia stores a greater quantity of fat than does any other representative of the species, the flesh being about 16 per cent fat. In Alaska this species has from 5 per cent to 10 per cent of fat, the average being 7 per cent, or less than half that of the Columbia River blueback. The question in regard to the imported sockeyes was: Is the quantity of fat stored determined by heredity or by environment? The former has been found to be the case. The imported sockeyes, although presumably living under the same conditions as the native bluebacks, still store much less oil than do the bluebacks. Samples of a second generation of sockeyes in the Columbia are now available. The samples have not been analyzed as yet, but from general observation the fat content does not seem to have been changed by the longer residence in the Columbia. Interest in this character lies in the fact that the value of the canned product varies directly with the quantity of fat. The imported fish, therefore, are greatly inferior to the native bluebacks.

Among the instincts of salmon the homing instinct has been of most concern. The fact that salmon return to spawn in the river system from which they entered the ocean is substantiated by a wealth of data from other sources. It will be sufficient merely to mention here that no fish marked on the Columbia has been recovered in any other river system. We shall be concerned here with a more exacting analysis of the homing instinct—the return to a particular tributary. The sockeye-marking experiments are not especially satisfactory for this consideration, because they involve transplanted fish and because the tributaries in which the fingerlings were liberated have not offered favorable conditions for the return of the adult fish. Nevertheless, there has been a distinct tendency on the part of the fish to return to the place of liberation. What straying has occurred has been mainly to the tributaries in the vicinity of the one in which the fingerlings were liberated. In one experiment, however, none of the adult fish returned to the place of liberation. This instance is of much interest to practical fish culturists. The hatchery at which the fish were reared was constructed primarily for the propagation

of sockeyes. It purposely was situated on a tributary in which native bluebacks formerly spawned, and an effort was made to simulate further natural conditions. The marking experiment conducted at that station resulted in the recovery of 2,500 adults in the commercial fishery, but none returned to the hatchery stream. Why they did not return to the place of liberation or where they did go is not known. There may be some significance to the fact that this particular race of sockeyes, in their native river in Alaska, has a migration route of only a few miles from the ocean to the spawning grounds, whereas the station at which they were liberated on the Columbia is approximately 500 miles from the ocean. The instinct to return to the place of liberation may have been present, but the fish may have been physically unable to make such a long migration. The small amount of fat stored by them may have been insufficient to supply the energy necessary for the long migration without food. The importance of this feature of the homing instinct lies in the fact that a hatchery can not become permanent in its operations unless it can produce its own breeding stock. Eggs can not be introduced indefinitely from some other locality.

The chinook marking experiments are too numerous and too varied in nature to be discussed in detail at this time. Only the most general conclusions will be given. From the standpoint of the number of returns, the chinook experiments have been far less successful than those with the sockeye salmon. The reported recoveries have varied from 1 out of each 50,000 fingerlings liberated to 1 out of each 300. The reported recoveries are not as representative of the actual number of returns as in the case of the sockeye experiments, but this discrepancy will not account for the great difference in the returns from the two species. The reasons for the difference in the returns is unknown. One of the chinook experiments was identical in every detail with one of the sockeye experiments. The fish were even reared together in the same pond, and the same mark was used on both species, but the return of the chinooks was only one-sixth as great as that of the sockeyes. It is hoped that future experiments, which have been designed to involve a greater variety of hatchery practices, may give better returns and point the way to more successful hatchery operations.

As with the sockeyes, the question of the quality of the flesh has been an important one. The chinooks of the Columbia vary in quality from a maximum for which canners are willing to pay to 13 cents per pound to a minimum for which they can pay only 2 cents. An effort has been made to determine if this character is hereditary or if it can be changed by changing the environment of the fish through artificial propagation. The results are not conclusive as yet, but the indications are that, as with sockeyes, the quality of the flesh is determined by heredity.

The homing instinct has been found to be the same with chinooks as with sockeyes. They instinctively return to the tributary in which they spend the early part of their life, whether this is their native tributary or one into which they were introduced. Their migration may be altered, however, by unfavorable conditions.

Another feature of the spawning migration that has been found to be hereditary is the time of year when the fish leave the ocean. Chinooks enter the Columbia during every month of the year, but fish having the same ancestry are to be found only during a definite and regular part of each season.

The knowledge of the nature of these two features of the spawning migration has thrown doubt upon the advisability of indiscriminately transferring the eggs from one tributary to another. If under natural conditions all or nearly all of the salmon return to spawn in their home tributary, it would follow that each tributary supports a separate colony or race of salmon. As a result, there would be a constant interbreeding between fish of the same colony and an absence of cross breeding between different colonies. This would lead to the development of racial differences through which the fish would become adapted to the conditions that prevail in their home tributary. We have found that character (of starting the spawning migration at a definite period) to be among these racial differences. If, then, the progeny of any race are transferred to a tributary in which different conditions prevail, the resulting adult fish may not be able to adjust themselves to the new conditions, and as a result they may not be able to deposit their eggs. Several marking experiments have indicated that this is possible. These experiments were with a race of chinooks that normally spawns at a considerable distance from the ocean and that starts its upriver migration three or four months before spawning time.

Fingerlings from this race were introduced into a creek that empties into the Columbia only a short distance from the ocean. When the resulting adult fish returned to the Columbia they lingered for a short time at the mouth of the creek but finally continued on up the main Columbia, with no definite destination before them. Whether or not they succeeded in spawning, we do not know.

As a converse experiment the progeny of chinooks, which normally leave the ocean late in the season and make only a short migration, were reared and liberated at a distance of several hundred miles from the ocean. The fish in these experiments have not yet reached maturity.

The opportunity offered by these experiments to secure scales of salmon of known age and life history has not been overlooked. It is hardly necessary now to present evidence of the validity of salmon-scale reading in general. It is pleasing, however, to find that without exception our interpretation of the scales has agreed with the known age of the fish.

The study of the chinook scales has clarified our understanding of many perplexing variations that are found in the scale history of the first year's growth.

Doctor Rich. The rather striking difference between the results obtained from the marking of the sockeye salmon in the Columbia River and the marking of the chinook salmon is worthy of an explanation. There are a number of reasons why we could not expect as great returns from the marking of the chinook salmon as were received from the marking of the sockeye. In the first place, they have different feeding habits in the ocean. The sockeyes are plankton feeders, and the chinooks feed on smaller fishes. The distinct difference in feeding habits is a factor presumably of some importance. In the majority of cases the chinook salmon have spent a greater actual mortality to take place with the chinook.
to the difficulties of ocean life for a longer time. The result is that while the mortality might not be at the same rate, one would expect a greater actual mortality to take place with the chinook.

Probably the most important fact that makes the difference in returns is that the sockeyes in the Columbia River are so different from the other fish that are running at the same time that they are separated and counted separately, so that much less difficulty is experienced in getting the returns from the sockeye. When there may be 30 or 40 tons of chinook salmon on the floor they will have a few hundred pounds of the sockeyes, and it is much easier for the men in the canneries to pick out these and get their dollar reward than it is to pick out the comparatively few marked chinook salmon that may be mingled with this great mass of other unmarked fish.

Mr. Bower. It has occurred to me that the Columbia River is one of the outstanding examples of the effects of fish-cultural work. Something that Mr. Holmes said leads me to suspect that he thinks the Columbia River is on trial in that respect. May I ask whether he thinks the work is a success on that stream?

Mr. Holmes. That is rather difficult. Practices differ so, and our experiments have covered such a small part of the work that the results may be so different that I would hesitate to give any opinion. I may say that from what I know of salmon culture, I really think the Columbia River hatcheries are at the top. The particular experiment with sockeyes in the Columbia shows that since we put our figures on the basis of the percentage of returns from the number of fish liberated, our reported returns were 1.4 per cent, the next were 2.5 per cent, and the third were 4.8 per cent. As Doctor Rich has pointed out, the conditions that determined the number, or rather the percentage, of return have not been particularly favorable. They have

varied in different years. In the first experiment the men were not aware of just how we wanted our returns. Butchering is done by the Chinese. We have an element there that makes it difficult to get results. We require rather detailed data regarding the size of the fish, time and place of capture, etc., and many of them felt that it wasn't worth while. One of my greatest tasks has been to devise ways and means of getting these returns in. I have been able to pick out one man in the cannery to look after things. After the butcher threw the fish out we gave him part of the reward and the picked man did the rest. Probably we have been getting better returns in recent years than before. I think the last experiment may represent conditions as they are. The several experiments are conducted with every condition identical, the only difference being in the fins that are removed and the variation in practice of the particular hatchery. The fish will return in the same year, at the same time, and at the same age. It is with experiments of this nature that we hope to have greater success in the future.

ALASKA HERRING

By George A. Rounsefell

The aim of this investigation has been to determine the following with regard to the Alaska herring, *Clupea pallasii*: First, whether or not depletion is occurring, either generally or locally; second, whether the fluctuations in abundance are due to natural causes or to overfishing, and the extent to which they can be foretold; and third, what protective measures are necessary to maintain an adequate future supply of herring.

The first problem to be attacked was that with respect to the homogeneity of the species. If the herring were a homogeneous population, migrating freely up and down the narrow coastal banks, the problem would take an entirely different aspect than if the herring were local; depletion, to occur, would have to be widespread; regulations, to be effective, would have to consider that fishing in one locality affected every other locality. On the other hand, if each locality supported a local race it would be possible for the supply in one area to be reduced greatly without affecting the supply elsewhere.

In order to determine whether or not there are such local races, we have made a study of the racial peculiarities. We have measured from the tip of the snout to the end of the occipital bone, to the end of the opercular bone, and to the insertions of the dorsal and anal fins, and have calculated what proportion of the body length each of these measurements is. Counts have been made of the number of dorsal and anal fin rays and of the vertebræ. These characteristics have been compared for several localities with illuminating results.

The total range of the vertebral count is from 45 to 57; the localities that differ the most (California and the Shumagin Islands) overlap so slightly that each overlaps only on the count of 53 and the difference between their means is 3.9, with a probable error of 0.06, making a difference of sixty-five times the probable error. The number of vertebræ tends to increase as one goes northward and westward from San Francisco Bay, up through British Columbia and southeastern Alaska, and across the gulf to central Alaska, and even continues to increase as one goes southwestward to the Shumagin Islands. However, it is a very surprising thing that a sample from Golovin Bay, Norton Sound, showed a decrease in number, so that it was about the same mean as Old Harbor.

The ratio between head length and body length seems to show the opposite tendency, the heads being much longer in southeastern Alaska than farther to the westward, the averages of 25 centimeters body length for fish from southeastern Alaska and Russian Harbor showing a difference of roughly 2 per cent, or a difference in actual head length of 5 millimeters.

The dorsal and anal fin-ray counts also show significant differences, although less pronounced than those of the vertebræ.

These differences may be hereditary or due to environment. If these structural differences are due to environment, they are probably fixed at a very

early stage of development—in fact, before the embryo leaves the egg. Any intermingling between the herring of different localities after these characters became fixed would cause all of these differences to vanish. The question might arise as to whether each locality was contributing to the others through the drift of eggs during the early embryonic period, before the characters had become fixed. The eggs themselves, however, are attached securely to the seaweed and eel grass, and so are not subjected to drift. These facts would seem to preclude the possibility that the population of one region contributes very extensively to that of another, and to show that the fishery is not drawing upon any great offshore body of herring.

The problem now is to refine the analysis to a point where it can be determined if differences occur between closely adjacent localities. This will necessitate a careful examination of larger numbers in order to arrive closer to the true meaning of the distribution of each character, and to obtain an accurate measure of the variation to be found between samples.

Supposing that, through a careful study of these characters, the differences give indications of the relative independence of the different grounds; the question still remains as to the reason for the changes in abundance. Are these changes natural or due to overfishing?

Great natural fluctuations in abundance occur in both the European herring and the California sardine, due to the presence of dominant age groups. Such a phenomenon may be one cause of the great fluctuations in the abundance of the Alaska herring. To determine this point, samples are being taken for length-frequency studies, and the ages are read from the scales.

The length-frequency distributions of herring taken in July in Prince William Sound for the past three years have been studied. These studies show a forward progression of the modes that may be caused by dominant age groups. This is also suggested by the last year's collections, in which the 5-year-olds are subordinate to both the 4 and 6 year olds.

In order to make certain as to this point, it will be necessary to collect data for a few years and note the changes in the position of the size modes. The collection of data of this sort presents some difficulties. There is question as to the size of samples and the frequency with which they must be taken in order to follow the changes in the run. As soon as the opportunity offers of obtaining samples for a complete season in one locality, a study will be made to determine how often samples must be taken and how large they must be in order to secure an adequate representation of the population.

If regulation is necessary, either because of overfishing or reckless use, there must be, necessarily, some knowledge on which to base it. What conditions should the fish be in when used? At what time do they spawn, and when do they reach a suitable condition for packing? At what age do the herring spawn? These and many similar problems are being investigated.

The only data obtained on the age at maturity were taken from beach-seined samples at Halibut Cove. No 2-year-olds were mature; out of twenty-five 3-year-olds, 13, or 52 per cent, were mature; out of twenty-four 4-year-olds, 20 or 83 per cent, were mature; all of the 5-year-olds were mature.

Weights were taken to follow the changes in the weight of herring of any given length, as an index to the condition in the various localities and at different seasons. Owing to the unusual scarcity of herring it was not possible to follow this throughout the season in one locality. The results show principally the need of data throughout the season in one locailty, but they have some value.

In order to have a standard of comparison, we have shown the relation between the weight and the length by using a condition factor obtained by the formula—K equals one hundred times the weight, divided by the cube of the length. It is expected that besides a difference throughout the season and between localities, there will also be an annual variation, not only in the time when a certain condition is reached but probably also in the maximum condition attained.

Taking herring of 255 millimeters body length a couple of weeks previous to spawning, at Halibut Cove in April, when the herring contained no fat but about 25 per cent of their weight was contained in the ripe gonads, the condition factor was 1.35. During the period from June 25 to July 2, in Prince William Sound at Elrington Pass, the smaller herring were in good condition and contained an abundance of belly fat, but the larger herring in the same samples contained very little fat and were rather thin, the condition factor at 255 millimeters body length being then 1.42. In the female gonads from one to half

a dozen ripe eggs often were found, and the male gonads had blood clots, and both the female and male gonads were flabby. All of these facts tend to show that it was probably not long since they had spawned. In Red Fox Bay, Shuyak Straits, from July 15 to August 5, the herring were in the very best of condition and the factor at 255 millimeters was 1.57. In Kachemak Bay, from August 19 to August 30, the herring were also in fine condition, the gonads weighing only from 1 to 5 per cent of the total weight and the belly still containing much fat, the condition factor was 1.56. In Eshamy Bay, Prince William Sound, from September 12 to 20, the herring were all rather thin, with only a trace of belly fat, and their condition factor at 255 millimeters was 1.34. A few were too thin to salt.

Mr. RADCLIFFE. I want to raise a question. There is considerable discussion as to how far it is desirable to use herring for commercial purposes, or whether we should use the herring at all. The herring is one of the best food-conversion plants that we have. Would it not be better to let members of the herring family stay in the water and furnish food for other fishes which are considered of greater economic importance?

Mr. ROUNSEFELL. That is a question that is very hard to answer—as to the conversion of food products. The salmon fisherman looks with disfavor upon the extensive use of herring in reduction plants because they believe that is why the pink salmon is becoming more scarce.

Mr. BOWER. I assume you take it that herring is just as important for a reduction plant as for food.

Mr. ROUNSEFELL. In southern Alaska very few of the herring are used for food. Some years ago the Franklin Packing Co. canned a great many herring and lost a great deal of money. Herring in southern Alaska are not good for anything but reduction into meal and fertilizer. Farther north they are given the "Scotch cure"—salted and used for food.

THE RAZOR CLAM

By DR. F. W. WEYMOUTH

[*Read by Dr. W. H. Rich*]

The razor clam is of wide distribution on the Pacific coast and is found in great abundance. It shares with the oyster and abalone the foremost place among edible shellfish in this region. In 1924, the value of canned razor clams was more than $863,000—about equal to all other shellfish together.

The habitat of the razor clam is strictly limited to broad, sandy beaches. They are accessible to diggers, and, due to their high value, are subject to such intensive fishing that many of the beds are evidently depleted. Information that will enable us to follow the abundance of the razor clam, detect signs of depletion, and point the way to intelligent protective measures is being sought. The wide distribution, under varied conditions, of this single species presents unique opportunity to follow certain biological problems of importance, which also have a direct bearing on the protection of the species.

As long as the industry retains its present value, certain facts should be gathered annually and made available for future comparison and for an index to the condition of the beds. A general survey of the industry, including the intensity of digging, area being exploited, and the size and age of clams in the catch, is essential. Observation of the spawning and the abundance of the resulting set gives an index to the future supply of clams. The relation of the set to number and age of the spawners, as shown by the commercial catch, should also be followed out.

Study of the rate of growth of clams from Oceano and Crescent City, Calif., Warrenton, Oreg., Copalis, Wash., Graham Island, British Columbia, and important beds in Alaska are being completed. Observations on the spawning

and other natural-history features are being made as opportunity offers. The abundance of the set on any bed determines the amount of digging that it will support, and efforts are being made to determine the success of each spawning.

The work has been carried on continuously since 1923. Special attention has been given to the beds most in need of protection. The ring method of age determination has been found to apply to this form. By the use of it, the course of growth for the more important beds and the age and size of sexual maturity have been determined. The relation of water temperature to the time of spawning has been studied on the Washington beach, as records from Alaska are difficult to obtain.

OYSTERS

GENERAL REVIEW—OYSTER SURVEYS AND EXPERIMENTAL PHYSIOLOGY

By Dr. P. S. Galtsoff

The purpose of the oyster investigations is to discover the method by which the production of oysters can be increased and better oysters can be produced. It sounds like a very simple problem; but, as you know, those problems that appear to be the simplest are often the most difficult for scientists. The solution of the problem requires a perfect knowledge of the organism and of the conditions under which it propagates itself and grows. This involves a study of the physiology of the organism and of the ecological conditions existing in the sounds, bays, and other bodies of water where the oyster grows in abundance. Thus, the investigation of the oyster fishery covers a great variety of physiological and hydrobiological problems. Geographically, this work extends from Cape Cod to the Gulf of Mexico.

I shall begin my review with the description of some physiological experiments that have been carried out during the past summer at Woods Hole. The first question that appears to be a very important one is that of the feeding of the oyster. Many attempts have been made by various investigators to determine the manner of feeding, but no accurate method has been evolved as yet. At the present stage of science no real progress can be expected unless a quantitative method is developed. We know, in a general way, that the oyster feeds on plankton carried in by the current produced by the gills. The first thing in this problem is to find out how much water is carried in and how many organisms are caught by the gills. The feeding of the oyster consists in three distinct phenomena: (1) Opening and closing of the shell, caused by the contraction of the adductor muscle; (2) production of currents by the ciliary epithelium; and (3) swallowing of the microorganisms caught by the gills. Only the two first phenomena so far have been studied.

The closing and opening of the shell were studied with a recording apparatus, which registers automatically every motion of the shell. The records show that on an average the oyster keeps its shell open for about 20 hours a day.

To study the currents produced by the gill epithelium a new method has been developed which enables us to determine accurately the amount of water that has passed through the oyster and to measure the pressure developed inside the gill cavity. It is also possible to collect the water that has passed through the gills and analyze it. Inasmuch as this method has been described in Science, I shall not enter into the details. It reaches its maximum (about 4 liters per hour) at 25° C. and slows down with the decrease of temperature. Below 7° C. no current is produced, though the cilia are still beating. This is entirely in accordance with the theory of hibernation developed by the bacteriologists who found that in winter, oysters taken from the polluted beds show a very low *Bacterium coli* score, while in summer the *B. coli* score of oysters taken from the same beds is very high.

By using my method, the water discharged from the gills can be collected and analyzed easily. Counting the microplankton in the tank water and in the discharged water, I found that more than 99.5 per cent of diatoms and dinoflagellates are caught by the gills. Water, after having passed the gills, contains almost nothing but mucus. With the cooperation of Doctor Pease's laboratory, I was able to make a few experiments to determine whether the bacteria are caught by the gills or if they pass through them freely. The following results were obtained: Water containing 27,000 *B. coli* per cubic centimeter contained, after passing through, 24,000 per cubic centimeter; another experi-

ment gave 16,000 and 11,000, and 9,000 and 7,000, respectively. These results indicate that a great many bacteria are able to pass through the gills.

The next important physiological question is the spawning of the oyster. Literature on the oyster contains many descriptions of the spawning habits of the oyster, but no real study of the phenomenon has been made as yet. The experiments were performed last summer with the oysters attached to the kymograph and kept in the tank where temperature, oxygen content, and pH were kept constant. The results of the experiments show that both male and female oysters can be induced to spawn by increasing the temperature of water in which they are kept. The kymograph records show that the spawning reactions of the male and female are different; in the first case the shell is kept open and the sperm flows in a continuous stream with the outgoing current of water; in the second case the rhythmical contractions of the adductor muscle cause the closing and opening of the shell. However, there is an additional factor that is responsible for the spawning of the female oyster. At a constant temperature the female can be induced to spawn by adding a small amount of sperm to the water. As the kymograph records show, the reaction lasts from 15 minutes to several hours, after which the oyster becomes insensitive to the subsequent addition of sperm. The sensitivity is restored, however, after two or three days. There is a certain minimum dose of sperm that produces the reaction. Two or three doses, each of which is less than a minimum dose, produce no reaction, though the total amount of sperm added is larger than the minimum dose. The oyster becomes immune, however, but comes back to normal condition after two or three days.

The results of this experiment have several practical applications: First, it shows that in order to obtain a successful spawning on the oyster grounds the oysters should lie rather densely, but not be scattered over the bottom; second, that when the temperature of water is low and the oysters do not spawn, they can be induced to spawn by adding sperm to the water. The latter experiment was tried in Milford by Mr. Prytherch and was successful.

Leaving the field of physiological research, we have to turn to some other work, namely, the study of the conditions under which the oyster grows. Investigations have been carried out during the last two years on Cape Cod, Long Island Sound, and the coastal waters of South Carolina, Georgia, and Texas; at present we are conducting investigations in Mississippi Sound, Miss., and Pamlico Sound, N. C. The purpose of these investigations, made at the request of the State authorities or representatives of the industry, is to answer the practical questions: How and where to plant oysters and how to utilize the natural oyster reefs and beds; how to grow better oysters? I am not going into the details of all the phases of this work, but I wish to make a general review of the conditions existing in the waters of the North Atlantic, South Atlantic, and the Gulf of Mexico.

The chief problem in the North Atlantic is the production of seed oysters. Set is getting scarce, and the production of a good set is the key to success in oyster culture. Extensive investigations and experiments on the behavior of oyster larvæ were made in northern waters. Last year artificial spat collectors were developed and tested in Great South Bay, Milford, Wareham, and Wellfleet. Instead of scattering oyster shells over the bottom, they were put into crates built of lath and planted over the various bottoms. Mr. Prytherch will describe the results of this experiment; it will suffice to tell at present that by this method the productivity of a given area of sea bottom can be increased from seven to ten times; and, besides, bottoms not suitable for ordinary planting, as, for instance, mud flats or sandy bars, can be utilized. In the southern waters the setting is so abundant that every object is covered with spat. This creates overcrowded conditions and results in the formation of oyster reefs or beds consisting in so-called "coon" oysters of a very little market value. Fortunately, in these waters setting seldom occurs below low-water mark, so that oysters planted there are not crowded out by successive generations and develop into a well-shaped product. The practical method of oyster culture in these waters is to obtain seed oysters on brush, then to transplant them into deep water. This method has been tried on a small scale in Georgia and has proved successful.

The study of the enemies of the oyster is of great importance. Out of many organisms that prey on oysters, the drill is the most dangerous and the most difficult one to combat. In certain localities, over 70 per cent of the oysters are destroyed by the drill. So far we have no efficient method

with which to combat this pest and know very little about the organism itself. We have decided, therefore, to include the investigation of the drill in our program. Doctor Federighi is conducting this investigation in Norfolk, Va., and will tell us about his plans and experiences.

COLLECTOR EXPERIMENTS

By H. F. Prytherch

As a foundation of this discussion I would like to describe conditions in the industry. On these we base all practical experiments and our study of the biology and environment of the oyster. The oyster industry in Connecticut represents the greatest development in the production of aquicultural crops. Here, we have man increasing production over nature by putting into operation his own methods and appliances. In some areas in Connecticut where oysters were formerly very prolific, to-day you can not find an oyster under any circumstances. From experimental plantings it was found that grounds out in deep water—30 or 40 feet deep—were suited to growing oysters. When they began transplanting oysters from inshore regions to these beds they occasionally got a crop of seed oysters, which led to the extensive practice of planting shells in deep water for seed collection. In a short time this practice failed to supply the industry, as conditions there are unsuitable for obtaining a yearly crop of seed oysters, because deep water in Connecticut is not the natural environment chosen by the oyster. It's choice has been the bays, harbors, and inshore regions, where food is more abundant and general conditions are more favorable. Milford Harbor was formerly a very prolific oyster-growing region, but practically no oysters were growing there when I first began studying; in fact, in 1925 we established two spawning beds in the harbor and put out several types of collectors. One type planted on the mud flats was birch brush; another type was glazed tile; and the third consisted in putting shells in baskets to see if oyster larvæ would go in there and attach to the old shells. The results of the first summer's work were unusually good. A good set formed on the brush and was obtained on the tiles also. The wire baskets, however, were the most practical and the best collectors of all. In 1925 the shells in these baskets were found to contain about 15,000 spat, so it was decided in the following year to carry on more extensive basket operations. In examining the baskets set out in 1925 we found that the larvæ had not penetrated into the center of the basket; so the following year we developed a triangular crate, which was stronger, cheaper to build, and gave much less distance for the oyster larvæ to penetrate. We found, at Milford Harbor, in putting out these crates, that oyster larvæ penetrated into the very center of the containers. The baskets put out in 1926 collected about 2,000 oyster larvæ, and though this was a smaller number than was collected in 1925, it was, at the same time, a good commercial crop. The difference in the quantity can be traced to conditions on the spawning grounds.

In experiments made with the crates in Massachusetts we found that the oyster larvæ attached only to the shells in the outer portion of the crate. In this case, scallop shells were used, which are smaller than the shells used at Milford Harbor, with the result that the oyster larvæ did not have the same chance of penetration. Controlled natural propagation has enabled us to use or restore these valuable inshore areas and utilize them for obtaining a maximum production of seed oysters. Conditions in this area are suitable, not only for seed collection, but also, for growing adult oysters.

There has been considerable comment lately with reference to artificial propagation as it applies to the oyster. I would like to discuss this phase a little, as I experimented with it for three years and understand the problems it presents. The fact that artificial propagation can be carried out successfully under laboratory conditions does not warrant conclusions that this method will be commercially practicable for the industry. However, I do not think that it will be possible ever to develop a method of artificial propagation that will produce in 10 years the amount of seed oysters that we produced in one summer in Milford Harbor. For example, in addition to the set on the crates, brush, etc., we collected over 100 bushels of seed oysters from a gravel bar in the harbor, averaging from 20,000 to 30,000 seed oysters to the bushel. That was only in an experimental way. To do that with any type of apparatus would be

an enormous task. Another thing with regard to artificial propagation is that it invariably succeeds when natural propagation succeeds and fails to produce results when results are needed most.

In order to carry on the experiments with the collectors it was necessary that we have wider knowledge of oyster larvæ and their behavior. For instance, in Milford Harbor, and in experiments carried out in Long Island Sound over a period of several years, we were able to collect the oyster larvæ when they were 2 days old, but could not find any trace of them again until they were within a day or two of setting. They are quite active a few days before setting, so that we were able to find many of them in this stage of development. In 1925, when Milford Harbor had been stocked and when spawning was known to have occurred, attempts were made to collect larvæ so as to determine their size and relative abundance. We were unable to find any except the smallest straight-hinge larvæ, and so we decided to take a sample of bottom mud or sand and see if we could find any trace of them there. In these samples we found quite a number of the larvæ in the intermediate stages of development, for which we had been searching for many years.

It is evident that the oyster larvæ in Milford Harbor spend a great deal of time lying on the bottom, and, in that particular body of water, conditions are such as to influence their behavior in this respect. The important point is that it gives us some knowledge as to the behavior of the oyster larvæ previous to the time of setting.

In bodies of water where conditions are more quiet we found the oyster larvæ throughout all stages swimming around in the water. In Milford Harbor the strong tidal currents make it necessary for them to protect themselves from being carried away, so they have developed this habit of settling to the bottom. To increase our knowledge of currents in Long Island Sound, we released 500 drift bottles. Some of the bottles made journeys of 50 miles in 5 to 12 days, and the majority moved great distances from places where they were placed originally. From the drift-bottle records it is obvious that if the oyster larvæ did not adapt themselves in this way to combat the currents they would be carried far away from the spawning beds, and oyster-cultural work would be even more difficult than it is at present.

Another great problem that has confronted us has been the peculiar attachment of the oyster spat in definite zones. In South Carolina and Georgia we found that setting occurred from low-water mark nearly to high-water mark; in Connecticut from the bottom of the channel to a point about 2 feet above low-water mark; and in South Bay, Long Island, from the bottom to the surface of the water. In South Carolina waters it was observed that the greatest intensity of the set occurred at a point midway between high and low water. Milford Harbor had its greatest intensity occurring at the level of low, slack water.

From experiments at Milford Harbor it was found that the predominating factor that controlled the attachment of the oyster larvæ was the velocity of the tide. The time at which the greatest attachment occurred was when the tidal velocity was practically 0. The tides in Milford Harbor are such that the oyster larvæ are unable to attach after the tide has reached a point about 2 feet above low water. At this point the tidal currents have developed a velocity of 10 centimeters per second, preventing the oyster larvæ from attaching themselves to any object. The type of tidal current in South Carolina and Georgia waters is different from that of Long Island Sound, with the result that the zone of attachment is different also. Of what significance is this knowledge to the industry? In South Carolina, though we found the oysters attached above low-water mark only, we knew that their absence below would not indicate necessarily that they could not live there, and so we took advantage of this condition by planting the seed oysters on beds below low-water mark, where successive generations are unable to attach and crowd them out.

Doctor Galtsoff mentioned the effects of sperm on producing spawning in the oyster. In Milford Harbor water temperatures were high during the first part of July, but the oysters refused to spawn; so on July 15 I stripped some oysters and released the spawn over the beds with the result that the spawning occurred and spread over the entire spawning bed. This method was also carried out in Long Island Sound on the oyster grounds. The oysters were ripe and well filled with spawn and it was decided that no harm would be done by stripping them to induce the others to spawn. Five bushels were stripped and the product pumped down to the oyster bed. The oystermen that

owned this particular bed said they got a splendid set, but we are not claiming that we produced it.

Another question that is always interesting and important is the value of the scientific work to the industry. From my observations made in the vicinity of Milford Harbor over a period of years we accumulated some knowledge as to the condition of the oysters each year and the hydrographic conditions on the beds in Long Island Sound. I found favorable conditions for a set in some of the inshore regions in 1925 and advised the oystermen to plant their shells as early as possible. The oystermen plant from July 15 to August 15 usually. This year they got their shells out as rapidly as possible, with the result that they were able to secure the early set on July 27 out in the deep-water regions which ordinarily would not appear until the middle of August. The best crop of seed oysters since 1914 was obtained that year.

Mr. Prytherch. I have here a few exhibits to be passed around. Here is some brush with oysters attached, which was planted in 1925 in Milford Harbor. The brush disintegrates or is destroyed by shipworms, with the result that the oysters attached to it break apart or can be separated as single specimens, whereas when they are collected on shells the oysters continually crowd each other as they increase in size.

Doctor Fish. I would like to ask Doctor Galtsoff a question regarding the food of the oyster. You stated that 99 per cent of the forms contained in the water are taken in by the oyster. Does this mean 99 per cent of the larger forms, or of the total?

Doctor Galtsoff. That really means of the the microplankton. The water used was taken out of the tank where the oysters are kept. Dinoflagellates and diatoms were the principal organisms present. I collected one liter of water after it had passed through the gills, and after examination it was found to contain nothing but mucus. All these small forms constitute the food of the oyster. There is some question as to how all these things are taken into the stomach and consumed. I am going to study more carefully next year what kind of food the oyster takes in. We know at the present time that the oyster has the ability to select its food according to its size. It may be that it can select its food according to the kind of food desired. It may be that some of the diatoms are rejected. An important consideration is, what propels the food particles to the mouth cavity and then separates them—rejecting some and accepting others. We have found a method by which we can measure the amount of food passing into the mouth cavity. We must find out if everything is consumed or if something is rejected, and what part of the food is desirable. I know of one large oyster bed—15 by 6 miles—in which none of the oysters are of value. They are very dark and can not be marketed, probably due to lack of food. When we know exactly what the oyster needs for its food, we can avoid this. We may supply some artificial food, as is done in France; but first it is necessary that we know the type of food as well as the quantity required by the oyster.

Doctor Fish. I was wondering if possibly out of the total amount of food there was a minimum below which no use was made by the oyster.

Doctor Galtsoff. Possibly that is the case. We have no positive evidence; in our experiments we found nothing to confirm this fact. It seems to me that every organism about 2 microns in diameter can pass through the gills easily.

Mr. Thompson. I would like to ask about the drills. Has anyone any idea as to how fast they grow? It seems to me that if the rate of growth is very slow, and they move very little, it might be possible to exterminate them.

Doctor Federighi. The rate of growth is one of the problems that will be taken up this spring when the drill begins to spawn.

Mr. Prytherch. Our observations at Milford Harbor indicate that the adult drills cover the shells with their capsules. They hatch at about the same time that the oyster does. It seems that these little yellow drills are just large enough to keep pace with the growing oyster. In Milford Harbor we found 60 of these tiny drills that had drilled something like 120 of the smaller oysters in one small area.

Mr. Thompson. If that is true, I should think you could study the problem of growth simply by a study of the length.

Mr. Prytherch. We very likely could, although we find all sizes in the sounds.

Mr. Gutsell. I would like to ask another question with regard to the oyster. As I understand it, the stimulation of the female to spawn is produced through the addition of sperm. Does that occur in the case of those that are unripe as well as those that are ripe?

Doctor Galtsoff. Oh, yes. The difference is that if the female is not quite ripe the reaction may last for 10 or 12 hours. This reaction has been recorded by a slow-going kymograph. In one case the reaction recorded required six hours.

Mr. Radcliffe. Is my understanding correct, Doctor Galtsoff, that the artificial stimulus to spawning might be used in Long Island Sound in certain seasons when there is no natural spawning? Might it be possible to produce spawning in years when otherwise no spawning would occur?

Doctor Galtsoff. Probably Long Island Sound is quite a good locality. I certainly believe that it could be done in Great South Bay and that the artificial impulse to spawn can be made of practical use.

Mr. Thompson. I would suggest that one of the best places to test that would be in California.

OYSTER-DRILL CONTROL

By Dr. Henry Federighi

Any remarks that I might make must be, necessarily, fragmentary. I have been working on the problem for a little less than three months, and most of that time has been spent in getting a laboratory ready, playing the part of carpenter, plumber, etc.—everything but a biologist. Recently I started the experimental work.

The oyster "drill" is a very important pest in the oyster industry. It is known that the drills have caused tremendous losses to the oyster business, but until recently the oyster drill had not entered the Chesapeake Bay—one of the largest centers of the oyster industry. The old literature contains some data on the drill. The older workers who made surveys would recommend that something be done to control the oyster drill. They found the drill in this locality and that locality. Florida is especially fortunate in having a specific drill of its own. The work is just beginning, of course, and so not much has been done on the problem. The only work I know of is that done by a man in the fisheries here—one named Pope—who, in 1911, made some experiments at Woods Hole. He worked especially on the reaction of the organism

to various food substances—oysters and clams. He placed the drills and some oysters, opened or unopened, in a tank. The drill invariably would migrate toward the oyster, whether the oyster was or was not opened, and proceed to eat it. This is about all we know about the oyster drill.

Of course, my talk is going to be about what I want to do instead of what I have done. I therefore believe it would be better at first to give a plan of the problem as I see it. The problem is very complex—in fact, the more you think of it, the more complex it becomes. The whole problem could be divided into two or three separate problems. The first thing to know is something about the anatomy of the organism, because structure is nothing but crystallized function. It is necessary to know something about the structure in order to know something about the behavior. The second thing is to state the natural history of the organism. By that I mean the fertilization, spawning, growth, migration, if any, and its distribution, etc. The third problem relates to the reactions of the organism to experimental conditions; that is, supplementing the field work with work in the laboratory—experiments in which we know most of the conditions and in which conditions can be controlled, so that under the same conditions we shall be able to predict the reaction. Doctor Galtsoff called attention to that—that ecology must be supplemented by laboratory experiments in order to understand the behavior of the organism. Then the last problem—to find a method of controlling the organism. It seems very probable that if something is known about the natural history of the drill, some way of getting rid of it will suggest itself. The organism must have some bad habit. I am interested in getting rid of the organism or in reducing its numbers to such an extent that it will be negligible. As the work progresses, new problems will arise, and some of the intended work will be neglected for more important work; but I believe the plan must go forward to obtain results. The work has been going on for 10 weeks. It is being conducted at Craney Island, a United States Public Health Service quarantine station. They have given me two rooms; one is heated and the other is not. The first part of the work consisted in equipping a laboratory and an aquarium room.

The oyster drill is a small gastropod, whose average length is about 1 inch. Larger ones occur, but only rarely. It is found everywhere where marine bivalves are found. It is distributed along the whole Atlantic coast. Up until two or three years ago the "drill" was almost unknown in the more inland waters (Chesapeake Bay). Recently it has invaded the more inland oyster beds and has caused great losses. From actual count, some beds have suffered a depletion of almost 80 per cent. In one bed, which contains 45,000 bushels, over 50 per cent of the oysters have been killed. It is a very serious problem. Between pollution and the oyster drill the oystermen are greatly worried.

The biological work so far consists of a preliminary survey of the situation in Chesapeake Bay and the neighboring rivers. The work has only just been started, and I have not enough data from which to derive any conclusion. I can say only that the work is in progress and that something can be learned from this work already. The survey includes the following factors: The abundance of the drills; whether or not egg cases are present; the salinity; and whether or not any oysters have been killed in that area. Of course, some observations are not very accurate—for instance, the presence of egg cases. I do not know whether the eggs were put there by the drill themselves or whether the egg cases were carried there from other areas.

Salinity is a very important factor. If you talk to the oystermen, they say: "What we want is about two weeks of good hard rain that will freshen the waters." It is believed that this would so reduce the salinity as to kill the drills.

Experiments on migration were attempted, but before I say anything about this I want to say something about the reactions of the organism within the laboratory. I have collected several hundreds of the organisms and have kept them in tanks in the laboratory. These animals were carefully watched while in the laboratory, and it was observed that the drills showed no activity. They seem to be in a state of hibernation, if there is such a thing. This was rather puzzling at first, and still is. This condition was thought to be due to the temperature of the laboratory. The running sea water was preheated by means of a small kerosene lamp. In that way I raised the temperature, but the organisms still refused to show activity. I then tried to see whether starved "drills" would respond to oyster and clam meals. No responses were

obtained. Newly collected drills gave the same results. Migration experiments will be conducted to determine whether the drills are inactive in the field.

Some work was done also on the phototropism of the organism, but no results were obtained. That is the extent of the work at the present time. I hope that as it becomes warmer more work will be done and more results will be obtained.

FISHERIES OF THE GREAT LAKES

GENERAL REVIEW

By Dr. Walter Koelz

The Great Lakes are a unique series of water bodies that have been formed by the blocking of ancient valleys. They have a very complicated history—now they were separated, now intimately joined. They have emptied through the Mississippi, the St. Lawrence, the Mohawk; they have even been flooded, in part, by the ocean, and all within relatively recent geological ages—that is, some 25,000 years.

The total area of the five lakes is in the neighborhood of 100,000 square miles. Lake Superior is the largest and deepest. The greatest depth is about 1,000 feet. Lake Ontario is the smallest and Lake Erie the shallowest, being less than 100 feet deep over most of its area. The lakes are what limnologists call first-class lakes. Their waters are very deep, very pure, and very cold, and few plants, except algæ, grow in them. Their economy is probably so different from that of small and shallow lakes that principles derived from a study of the smaller lakes may not be applied to the culture of the Great Lakes. In spite of the apparent sterility of their waters, the fish production has been high. The total annual yield of the commercial fisheries has averaged around 150,000,000 pounds. The productivity of the individual lakes, however, is not equal, due to the varied conditions that obtain in them. Lake Superior and Lake Ontario are the poorest, averaging 500 and 650 pounds per square mile per year; Huron comes next, with 800; Michigan has 1,100; and Lake Erie has far exceeded the others combined, with about 6,000. These relative figures do not reflect the effects of depletion (it is assumed that depletion has been relatively equal in all), and the inequality of the figures undoubtedly is a reflection of the unequal productive capacity of the various bodies of water. Favorable conditions for fish do not depend alone on humidity, as many people, including scientists, seem to think, but on bottom conditions, temperatures, etc.; and the fishermen know well that there are hundreds of square miles in the lakes that produce nothing.

Of the grand total of 150,000,000 pounds, about one-half consists of species of Coregoninæ—the whitefishes and lake herrings. These fish occur throughout the boreal regions and are among the world's most important fresh-water fishes. Wherever they occur, however, it has been very difficult to define the limits of a species, so that it is not known, even approximately, how many species there are in the world, or, in fact, in any region. This has been no less true in the Great Lakes. Where other fishes are separable by color, number of fin rays, vertebræ, scales, proportions, etc., the various species of these fish are alike in all these characters.

It is necessary obviously to determine what forms occur and to define these forms before anything can be done to conserve the fisheries that they sustain. This uninteresting undertaking of describing the physical differences of species is what the average worker understands as systematics. Systematic workers are in more or less disrepute among the workers in practical science, and it is more or less just that they should be. Too many of our systematists have failed to realize that describing and naming species or subspecies or races is not an end, but only a beginning, and that no one should or may safely name or describe any form until he is acquainted with the important facts of its life history and that of its relatives.

My work on the Great Lakes, then, has of necessity attempted to correlate differences in structure with differences in habits. It is of no biological significance that a human being should find that a fish is very unlike another fish, if the fish feel intimately acquainted with each other and are able to spawn together; nor does it alter matters if fish and fish are exactly alike, so far as I can see, if they have totally different physiological reactions and never spawn together. The Great Lakes whitefishes illustrate, abundantly, both kinds of phenomena. The lake herring in most of the lakes exist in two forms—one

very elongate and terete in form, with a body depth of one-fifth its length, and the other with a short, much compressed body, with a body depth of one-third the length. Both forms may spawn together. About 10 specific names have been given to this single species in the Great Lakes.

In Lake Superior two species resemble each other so closely that they can be distinguished only by the value of the fraction, which for numerator has the sum of head depth and base of the anal fin and for denominator the sum of the maxillary and snout; and even these values, for the two species, are not exclusive. The fish are, nevertheless, very distinct in their habits. One spawns in September in deep water; the other in November along the shores. So much for structures.

Even the habits of the various races of a species may not be alike in different areas. One species in Lake Michigan spawns in May, while in Lake Ontario its representatives, which can not be distinguished from their brethren in Lake Michigan, spawn in January. Even in a restricted locality, one school of the true whitefish may spawn on one reef in October while another reef, a few miles away, may not be visited by spawning fish until December.

Where such variability exists, members of a species may differ more from one another than from individuals of other species; two species may be almost exactly alike in their structural features and the breeding habits of races of the same species are entirely different.

I would not have you believe, however, that the situation remains so obscure and confused that no one but me will ever be able to identify the Great Lakes whitefishes. I have been able to send descriptions of desired species to various fishermen and have received in return collections of the fish I wanted. It must be apparent, also, that it is possible to describe the fishes in simple terms, for no fisherman would attempt to interpret the descriptive formulas that systematic ichthyologists have found necessary to employ in portraying a species.

The coregonids, like most of the other Great Lakes fish, are taken by the fishermen with gill nets and traps. Most of them have been the object of commercial fisheries for more than 50 years. Until their numbers were so seriously depleted that it became apparent that some protection must be granted, fishing was unrestricted. The fishermen were allowed to catch fish of any size, in any quantity, at any time. Even after protective laws were put on the statute books, usually they were not enforced immediately. In late years some kind of minimum-mesh laws have been enacted and usually enforced, and in some places one species—the whitefish—has been more or less protected in the spawning season. These fish thus have sustained an almost unrestricted fishery, whose effects have been tempered only by the "law" of supply and demand. Fortunately, the supply has, until recently, exceeded the demand. In 1925, however, the impossible, the incredible, happened. The herring of Lake Erie, suddenly and without warning, gave out. Where in 1924 the fish could be caught by the ton, in 1925 only stray individuals could be found, though dozens of boats, with miles of nets, set virtually from top to bottom in the lake, undertook the search. In 1926 the fish still failed to appear. A fishery that annually yielded as many high-class fish as could be sold (around 25,000,000 pounds, about one-sixth of the Great Lakes total yield) was no more.

It has been mentioned before at these meetings that though the Great Lakes yield has been maintained at about a constant figure, it has been achieved only by the capture of undesirable species to replace the choice ones that have become rare. The Great Lakes have now no more unexploited species, except the shiners that throng the shores, and the loss of one-sixth of the production of the choicest fish marks the last chapter in the history of a once important industry. The herrings everywhere in other lakes have been sought to fill this gap in production. These species probably never have been collectively numerous enough to supply so great a quantity of produce, and in their seriously depleted condition the intensive fishery to which they are consequently subjected must unavoidably exterminate them also. One species is already extinct in all but one of the lakes. It follows, further, that the demand for species of fish other than coregonides will increase as they decline, and the fishery for these species must then become still more intensive, with possibly catastrophic consequences.

The most significant lesson that depletion has taught in the Great Lakes is that species once reduced never recover. Furthermore, it is not clear how the

blackfin, or the Erie herring, could ever have become exterminated with the employment of as large a mesh as was used to take them.

Theoretically, it should be quite all right to carry on a fishery that takes only the largest fish, especially when those fish already have spawned several times before their capture. We have indications that there is a delicate balance in the economy of gregarious species, which is adversely affected by great reduction in their numbers.

Besides "catching out" the fish, the waters in many once very productive areas have been made unfit for fish by the increased dumping of pollution from industrial sources. It almost comes to the condition that the fishermen sometimes charge that the fish are bound to be poisoned, anyway, and they might as well catch them as soon as possible.

As discouraging as it is that the fish are being exterminated and the waters polluted (and here I want to repeat that I am not an alarmist; the facts are plain and point to an unavoidable conclusion), it is demoralizing to realize that nothing is being done about it. No less than nine governments are administering conservation legislation, and it goes without saying that no two have the same idea! Some, in fact, have no idea at all! Control of these fisheries must be coordinated, and this clearly is possible only through a centralized body. As the waters are international, this body must be international in character.

The problems that confront other fisheries, confront the conservationist on the Great Lakes—we want to know more about the life histories of our species; we want to know whether artificial propagation does any good; we want adequate statistics; in short, we want to know everything about them, but, most of all, we want regulation of the quality and the quantity of the apparatus, or no fish will remain for us to investigate.

LIFE HISTORIES OF THE COREGONINÆ

By Dr. John Van Oosten

When the life-history studies of the coregonines of the Great Lakes were begun in 1921 the scale method of study had not been applied to these species of fish. Since then two short preliminary papers have appeared—one on the whitefish of Lake Erie and one on the herring of that lake—in which the scale method was employed. During the past six years I have accumulated many data on, and a large amount of scale material of, the lake herring, whitefish, blackfin, and several species of chubs. The chubs have been studied little. The work has been confined chiefly to the lake herring and to the whitefish of Lake Huron.

The problem, as I saw it at the beginning of the study on the lake herring, involved three questions: 1. Are the structural characters of the herring scales so clearly recognizable as to permit their study by the scale method? 2. If the characters are thus recognizable, are the fundamental assumptions underlying the scale method warranted in the lake herring? 3. If the scale method is valid, what does its application to the lake herring show concerning its life history and the fishery of it?

It is apparent at once, to those who are familiar with scale work, that the scope of the above problem is very broad indeed. I am convinced, however, that such a broad scope was entirely warranted. Too many life-history papers, based on the scale method, are written without any regard to the validity of the method, in so far as the particular species involved is concerned. It is repeatedly asserted by scale investigators that assumptions that are valid for one species are not necessarily valid for others. Further, for no one species of fish have all the assumptions of the scale method been verified, nor have these assumptions been tested critically in the wild, fresh-water fishes of this country. Most of the theoretical work has been done on the marine fishes—salmon, herring, and plaice.

The major assumptions whose validity was considered in my studies involved the constancy in the number and the identity of the scales throughout life, the growth relations between the scales and the body, and the relation between the formation of the annuli and the increments of time. Nearly 4,000 specimens, collected at the same place for four consecutive years, were employed for this study.

The various phases of the scale theory can not be considered at this time, but I would like to mention briefly two of my conclusions which affect the scale method as it is generally applied to-day. First, my data show that the

computations of length, based on the diameters of scales, are more accurate than those based on the anterior radius, the dimension now universally employed. All my computed data on the growth of the coregonines are based, therefore, on diameter measurements. Second, the so-called "Lee's phenomenon of apparent change in growth rate," a characteristic of the computations of length of virtually all species of fish for which calculations have been made, is due to perfectly natural events in the life history of the lake herring, and no corrections for it should be made. Briefly, Lee "phenomenon" is this: When we compare the calculated lengths derived from the scales of the various age groups of a collection, or of a year class, with one another, we find that for corresponding years the lengths computed from the scales of old fish are, nearly always, lower than those calculated from the scales of young fish; that is, the amount of calculated growth at corresponding ages increases regularly as the scales used are taken from fish of younger age groups. Thus, if the first year's growth increment is calculated from the scales of a 6-year fish, it is less than if calculated from scales of a younger fish. This is true, even though the fish belong to the same year class, that is, hatched in the same year and grown up under the same environmental conditions.

It is now generally believed that Lee's "phenomenon" is caused by the fact that scales do not begin their development until the fish has grown to a certain length. The growth history of the early part of the first year of life is not registered on the scales, therefore. This, if ignored, presumably introduces an error into the computations of length, which are based on the assumption that the entire history of the growth of the body of a fish is registered faithfully in its scales. On the basis of this belief, certain investigators apply a corrective formula to their computations of length.

My herring data, however, show that this corrective formula does not eliminate the "phenomenon," although it does become less pronounced; and that the length values, after having been corrected by this formula, appear to be too high for the early years of life. Further, the discussion shows that the corrective formula does not take into consideration the rapid growth of the scale, as compared with that of the body, especially the rapid growth occurring immediately after the scale appears during the first year of life. The effect of this relatively rapid scale growth upon the calculated lengths is exactly the opposite of that produced by the late appearance of the scale, and the former neutralizes or counterbalances (at least, in part) the latter.

Finally, the data show that the "phenomenon" not only characterizes the computed lengths but also the direct measurement of the scale diameters. That is, if we measure the annular diameters (the diameters included in the annuli) of the scales of the several age groups, of a year class, or of a sample, we find that for corresponding years these diameters in the scales of the old fish average less in length than those in the scales of the young fish—corresponding diameters decrease progressively with age. Obviously, tardy scale formation can not be a factor of the "phenomenon" found in the direct measurements of scale diameters, inasmuch as in them no computations are involved.

I may state, incidentally, that in the lake herring, Lee's "phenomenon" may be explained best as the result of four natural events in the life history. They are, briefly, as follows: 1. The late-maturing fish of a year class are the more slowly growing individuals of their year class. 2. The scale grows relatively faster than the body. 3. Sexual maturation retards the rate of growth. 4. A compensation in growth occurs—that is, fish that grow slowly during the earliest years of life grow rapidly during the later years of life, and vice versa. Lee's "phenomenon" is a natural event—it should be present in the herring calculations of length and no correction should be made for it.

In general, I found that the assumptions underlying the scale method were valid in the lake herring, and that the method may, therefore, be applied with confidence in a study of the life history of this species. The natural history of the lake herring is, briefly, as follows:

Lake herring may be taken at almost every port on Lake Huron. Saginaw Bay ranks first in the herring industry of this lake, while Alpena ranks second. Herring are taken in either pound or gill nets (in trap nets rarely) in the fall and spring of the year (the big catches are made in the fall during the spawning run). After June, most adult herring have moved out of the shallow water.

The herring occasionally take their food from the bottom, though they are typical pelagic feeders, subsisting principally on the plankton forms.

They spawn chiefly in November in shallow water (3 to 8 fathoms), preferably on a sandy or gravelly bottom. Very little is known about the young herring and many special efforts to obtain them have met with failure. As the herring generally have not been kept separate in the statistical reports, no definite information concerning their relative abundance is available at present.

The eggs of the herring deposited in the fall sometimes hatch in the early spring, probably April. During the first year the young herring grow very rapidly. On the average, they reach a length of 5 inches in the first year, 7.3 inches in the second year, 8.6 inches in the third year, 9.3 inches in the fourth year, 9.6 inches in the fifth, 10.2 inches in the sixth, and 10.8 inches in the seventh year; or, in terms of increments, they grow, during each year of life, beginning with the first year, 5, 2.3, 1.3, 0.7, 0.3, and 0.6 inches, respectively. If the length at the end of the seventh year is taken as 100 per cent, then the percentage of total growth completed at the end of the first year is 46.4, the end of the second year 67.5, at the end of the third year 79.6, at the end of the fourth year 85.8, at the end of the fifth year 89.1, and at the end of the sixth year 94.2. The herring complete nearly 50 per cent of their growth in length during the first year of life. Males and females grow at identical rates.

The growth history, in terms of weight, is as follows: At the end of the second year the herring weigh about 3.53 ounces; at the end of third, 5.03 ounces; at the end of fourth, 5.61 ounces; at the end of fifth, 6.18 ounces; and at the end of the sixth year, 7.78 ounces. In the third year the herring gain 1.5 ounces in weight; in the fourth, 0.58 ounce; in the fifth, 0.57 ounce; and in the sixth year, 1.6 ounces. If the total weight at the end of the sixth year is taken as 100 per cent, then the percentage of total weight attained at the end of each preceding year is as follows: Year second, 45.4; year third, 64.7; year fourth, 72.1; and year fifth, 79.4. The females weigh slightly more than the males at corresponding ages, but this presumably is due to the slightly greater weight of the partly developed female sex organs. In comparison with length, the rate of the proportional total-weight increase is small during the first years of life, for while more than three-fourths of the total length reached by the species is attained at the end of the third year, more than five years are required for a similar increase in weight. In this connection, it may be of interest to know that Gilbert's law of compensation in growth also applied to the lake herring. That is, it is found that, on the average, the big yearlings of a year class were the big fish of that year class in all succeeding years of life, but that the differences between the small and large yearlings diminished in each year of age—that is, the small yearlings were rapid growers, the large yearlings slow growers.

We do not know where the herring stay during the first two or three years of life. They do not appear in the commercial catches until their third year, when many attain sexual maturity. Very few second-year fish are taken in the commercial catches (0.2 to 0.8 per cent). Likewise few eighth-year fish are taken (0 to 0.4 per cent). The percentage of seventh-year fish present varied from 9 to 1.8 per cent; that of the sixth-year herring from 1.8 to 9.9 per cent. The commercial catches are almost entirely composed of third, fourth, and fifth year fish, these age groups comprising 87.2 to 97.4 per cent of the entire catch. In 1921 the fourth and fifth year fish formed the bulk of the sample—72.9 per cent of the total. In the three succeeding years (1922, 1923, and 1924) the third and fourth year fish predominated, representing 78.4 to 84.8 per cent of the sample. In each year the fourth age group was the largest, its individuals comprising 42.8 to 58.3 per cent of the total catch. I found that a shift occurred in the age composition; the percentage of third-year fish increased each year during the period 1921–1923 (14.3 to 32.8 per cent), and then remained stationary in 1924 (32.7 per cent). This general increase occurred at the expense of the third-year fish mainly, which each year became progressively less abundant (30.1 to 19 to 17.3 to 11.8 per cent).

In many species the commercial catches are dominated by one particular year class for two or more consecutive years. This does not occur in the lake herring. Each year class drops off rapidly in the years following the year of its dominance, which, as shown above, was the fourth. Every year a new year class predominates in the commercial catches.

In general, the males and females are equally abundant. Of 2,950 herring, 49.5 per cent were males and 50.5 per cent females. The relative abundance of each sex does vary, however, during the course of the spawning run; the males are more numerous than the females early in the season but less numerous late in the season.

For a long time I was puzzled by the peculiar growth history of the Saginaw Bay herring. Both actual and calculated measurements of length showed that these herring were becoming progressively larger at corresponding ages each successive year. Analyzing the growth data, I found that this increased growth rate began suddenly in 1919 and involved fish 3 years of age, or younger, only. This accelerated growth rate continued in the years 1920, 1921, and 1922. During this period, 1919–1922 the second and third year fish did not increase their growth rate but simply maintained the rate attained in 1919. The first-year fish, however, grew more rapidly each successive year. There was a progressive increase in the size of the first-year fish each successive year in the period 1919–1922. The data show further that in each of the years 1915 to 1918, inclusive, the rate of growth was relatively low for herring 3 years of age and younger, and that higher rates prevailed before 1915. The growth rates prevailing among the herring of Saginaw Bay before 1915, were in some way inhibited during the period 1915–1918, and restored or partly restored in 1919. It is shown that the alteration in the rates of growth was due primarily to some local changes in the environment of Saginaw Bay.

That the third year and younger fish only were affected is explained on the basis that only these age groups grow while in Saginaw Bay. The first year fish spawned in the bay presumably spend most of the first year in the bay, while the older fish spend only the early part of the growing season there; but if all fish older than 1 year were subject to the same environment, why, then, did the second and third year fish show an alteration in growth rate, while the fourth-year and older fish did not? This is explained on the basis that (1) the younger herring commence the new year's growth earlier in the spring than the older fish, and (2) that, as the annual growth becomes progressively less with age, slight alterations in growth rate can not be detected as readily in the average measurements of the older fish as in those of the younger.

The question then presented itself: What controlling factors operated in Saginaw Bay during the period 1915–1918 that were absent in the open lake? Did these factors exist, also, before the period 1915–1918, and were they absent or reduced subsequent to it? The following natural factors were considered: Temperature, sunshine, and fishing intensity. It was concluded that these factors did not control the unusual growth rates of the Saginaw Bay herring. A fourth factor proved to be, in all probability, the effective one, viz, the temporary chemical pollution of Saginaw Bay during the World War. In the spring of 1915 the Dow Chemical Co., of Midland, began to pour large amounts of paradichlorbenzol (a useless by-product) into the Saginaw River. This pollution became so severe that many of the fish in the Bay became tainted to such an extent that they could not be sold or used for food. This chemical pollution continued to taint the fish until the spring of 1918. So far as we know, 1919 was the first after 1915 in which the waters of the bay were entirely free from the Dow chemical pollution. It is seen, then, that the period 1915–1918, during which dichlorbenzol wastes polluted the bay, is precisely that during which the growth rate of the herring was retarded. Further, it was shown that the presence of this pollution explained all the facts in the growth history of the herring, and no known fact was inconsistent with this explanation.

The loss to the fisheries during 1915–1918 must have been considerable. Not only did the fishermen lose through the nonsalability of part of their products, but also through the deleterious effect of the pollution upon the growth rate of the fishes. The indirect loss occasioned by the latter factor is commonly disregarded as of little consequence, not only by the general public but by the fishermen themselves. This attitude can be accounted for by the fact that in most cases the magnitude of these indirect losses must be left to the imagination or be stated in terms of description instead of dollars and cents. In order to stress as emphatically as possible the importance of the indirect effect of pollution upon fish life and industry, I have computed roughly, from my growth data and herring statistics, the indirect financial losses suffered by the herring industry of Saginaw Bay during the affected period, 1917–1923:

Year	Pounds	Value	Year	Pounds	Value
1917	869,124	$26,074	1922	113,648	$3,409
1918	1,332,909	39,987	1923	3,191	96
1919	1,317,995	39,540			
1920	628,907	20,754	Total	4,450,224	136,685
1921	184,450	6,825			

Not only were the herring affected by the pollution, but presumably also the pickerel, perch, suckers, carp, and all other species of fish that grow in Saginaw Bay; and the total damage done to these species involved much greater financial losses than those of the herring, for they yield the bulk of the commercial catches of the bay and possess an average value greater than that of the herring. The significance of a pollution such as described here is seen in the fact that the herring industry was still affected in 1923—five years after the pollution ceased.

Finally, this study shows that from the point of view of the commercial fisheries, it is not profitable to allow the herring, at their present rate of growth, to become much older than four years, as they increase slowly in weight after the third year. Further, relatively few third-year fish should be taken. This will permit most of the herring to spawn once, and some twice, as virtually all the individuals of a year class reach sexual maturity in their third or fourth year of life.

If present conditions remain constant, the herring fishery does not appear to be in any immediate danger of extermination, in spite of the fact that no closed season exists for this species. This, no doubt is due partly to the fact that the intensive fishing is carried on during the spawning run when mature fish only are taken. Many of the immature second, third, and fourth year fish are thus unmolested. Further, many of the mature third-year fish escape the nets and therefore have a chance to spawn twice. But the handwriting is appearing on the wall. That the herring fishing is very intense is attested by the paucity of old individuals in the catch and by the shifting in the age composition. Relatively very few fourth-year herring escape the nets to comprise, a year later, the fifth-year group. The data show that commercial fishing is so intense that a year class is practically wiped out during its year of dominance—the fourth. Briefly stated, the history of the majority of individuals of a year class seems to be as follows: They are spawned in the fall, hatched in the spring, grow as immature fish for two or three years, attain sexual maturity in the third or fourth year, and are captured by the fishermen before or during their fifth year. Each year class is depleted rapidly in the year of its dominance.

Captain WALLACE. With regard to the pollution of the Great Lakes, I wonder if any account is being taken of the ashes that are dumped overboard by the steamers that have used the Great Lakes for a number of years. I have heard fisherman say that the quantity of ashes dumped overboard in Lake Erie and Lake Michigan must have some effect on the vegetable growth at the bottom.

Doctor VAN OOSTEN. There is no effect, as far as I know.

Captain WALLACE. I was wondering if it had been taken into account with regard to pollution?

Doctor VAN OOSTEN. Yes; it has been considered in regard to pollution.

Doctor FISH. For the most part vessels follow more or less definite routes, and the accumulation of ashes would be more or less along these routes. I should think that the bottom accumulation would be in the greater depths of water, where the only plant or animal life to be found would be the diatoms gotten in the open waters.

Doctor VAN OOSTEN. The places where the growth of plants is thickest are near the waters edge, and this water probably would not be affected by the ashes. The places where these ashes are dumped may not be the vital places of plant production.

Mr. RADCLIFFE. Is it not true that these cinders, etc., do shift around on the bottom on the spawning areas and are actually destructive of eggs and young fish?

Doctor VAN OOSTEN. There is no question but what anything that sinks to the bottom becomes deleterious to spawning. There are a great many species that never come off of the bottom at all—they get their food and all there.

Mr. Thompson. Mr. Scofield and I were asking each other why these herring in Lake Erie disappeared as suddenly as they did. In my talk on the first day I mentioned the fact that fishes might be adapted to meeting the emergencies of adverse periods, and that in the case of some species a certain minimum number might be required in order to survive. If you reduce the members of this species, you might go on for years with the supply perpetuating itself; but, on the other hand, an emergency might arise and your species would not be equal to the occasion and would disappear. It seems to me that that is not merely a theoretical condition but a practical one. If you reduce the abundance of a species so far, some day an emergency will arise which your species can not meet. This disappearance of the lake fisheries might possibly represent just such a case.

Doctor Koelz. That is nothing new biologically. The same thing has happened in the case of the bison. There have been similar cases in Europe.

Doctor Bigelow. It seems to me that it is also necessary to take into consideration the fact that more than one species of fish has suddenly become abundant in certain areas, as well as disappeared. In the nineties the bluefish became very abundant in New England. No one wanted it. Large catches were made because they could not prevent the fish from getting into their weirs. This sudden increase in supply has occurred a great many times, apparently without the aid of fishery agencies. I think that this point should be taken into account.

FISHERIES OF THE MISSISSIPPI RIVER

GENERAL REVIEW—WORK OF THE FISHERIES BIOLOGICAL STATION AT FAIRPORT, IOWA

By Thomas K. Chamberlain

The work of the bureau for 1926, carried out through the Fairport fisheries biological station, may be divided into four main groups of investigations: (1) Mussel investigations; (2) pond studies; (3) a river-pollution investigation that was only partly bureau work; and (4) a study of the influences of the Keokuk Dam upon the fish and mussel populations of the Mississippi River above and below the dam, especially in relation to their migrations.

This last is a resumption of an old investigation begun by Doctor Coker some years ago and which is now taken up again. Naturally, it covers a field wider than Keokuk Dam. It is hoped to make this the beginning of a general study of the influence upon fish populations of impounding waters and constructing dams. In this connection, the statement made by Doctor Rich, that a means was being developed to permit salmon to pass dams, may be of interest. Keokuk Dam has no fishway. Fish wishing to go above the dam are expected to use the lock.

Of the mussel work, that done by Doctor Ellis, of the University of Missouri, who has worked at Fairport several summers, unquestionably is the most important performed at Fairport during the past year. No doubt all are sufficiently acquainted with the life history of the commercial fresh-water mussel to know of the parasitic stage on the gills of fish, which the embryo mussels, or glochidia, must undergo in order to complete their development into mature mussels. During the past summer Doctor Ellis completed the preparation of a nutrient solution that serves as well as the living fish as a medium of completing this development.

Of course there is no use in painting too rosy pictures of what this may mean eventually, but there is no question but that this discovery will assist greatly in the work of propagating mussels. I believe that present methods of mussel propagation are of much value, however. As you may know, the millions of fish that the bureau's rescue crews save annually from the slowly

drying pools and sloughs along the Mississippi, where they are left by high water, are inoculated with glochidia and released. However, there is no control of these fish after they are released. We have no reason to believe that any very large proportion of the young mussels are dropped by their hosts in localities where they will survive, nor that they will escape their enemies. and passing destructive physical conditions during the first 10 days or 2 weeks of their free state—an extremely critical period in their life. However, button manufacturers and many others are convinced that the marked increase in abundance of the more valuable shells in certain areas is due to the bureau's activity.

The culture of mussels in troughs at Fairport has been going on for years in a small way by inoculating fish and permitting them to drop the young mussels in troughs, where they may be cared for through the first season of their growth. The number of glochidia that may be cared for by one fish is limited, however, making it quite an elaborate operation to obtain many mussels that way. Inoculating the maximum load of glochidia upon a fish also is a strain upon the fish. The mortality among the fish used in the trough culture of mussels has been heavy, and there is reason to believe that to a less extent it is heavy among the inoculated rescued fish also.

Doctor Ellis's work, at one stroke, does away with the necessity of inoculating rescued fish, and at the same time permits planting young mussels in large numbers upon mussel beds, where they may do best, and at an age when they have passed the early critical period.

While Doctor Ellis is confident that the main work is done, it is necessary for him to develop a bactericide that may render glochidia free of bacteria when placed in the nutrient solution, but which will not injure the glochidia. It is necessary that the glochidia be sterile when placed in the solution, otherwise bacteria that may be present will increase sufficiently rapidly to destroy the glochidia before metamorphosis is quite complete. Doctor Ellis expects to complete this work this coming summer.

Doctor Ellis also undertook for the bureau some work on the life history of the two species of gar found at Fairport, in connection with a study of the life history of the yellow sandshell (the most valuable of all fresh-water mussel shells), which is parasitic upon the gar. Bearing in mind Doctor Ellis's nutrient solution this fact may not be important, but for a better understanding of the yellow sandshell it has seemed best to study the habits of the gars.

The trough culture of mussels was continued at the station on a small scale during the past summer. Some new ideas were tried, which it is believed may be used to advantage in Doctor Ellis's system as that is developed. The juvenile mussels obtained were planted in certain Virginia waters by H. O. Hesen, who had charge of an investigation of the results of previous plantings of commercial mussels in these waters. No indications that these mussels were becoming established were obtained.

As a result of correspondence between the bureau and the game and fish commission of Arkansas, a cooperative plan was made for a survey of the mussel-producing waters of Arkansas, with a view to working out a satisfactory open and closed system for areas of mussel waters. A previous system failed because the areas were too large.

Ample proof had been obtained that Doctor Coker's plan of dividing mussel waters into unit lengths and closing every other unit, with the understanding that at the end of given periods the open and closed sections should be alternated, was highly efficient for conserving mussel resources. Both the bureau and the commission, therefore, were anxious that a workable plan embodying these features should be put into effect in Arkansas. I was sent to Arkansas in May, and with members of the commission covered virtually all of the mussel waters of the State in about three months. Points that would serve as divisions between open and closed sections were studied carefully and a plan finally was worked out and presented to the commission. As recommended, the plan was first published in State newspapers and criticisms requested. As the result of suggestions some minor changes were made and the plan goes into effect in February, 1927.

The station has been making a series of surveys of certain mussel areas in the upper Mississippi waters, especially Lake Pepin, a widened portion of the Mississippi lying between Minnesota and Wisconsin. It was intended that these surveys should give an adequate idea of the extent of the mussel resources of these areas and indication of any trends in the supply. Up to the present

time no entirely satisfactory method for making these surveys has been developed.

It was a little late in the summer when I reached the northern waters for the survey. The few good days were spent in attempting to develop a system that would compare favorably in accuracy with the surveys of Pacific coast clams made by Doctor Weymouth. Apparently, in Lake Pepin at least, going down in a diving helmet and removing all mussels from units of the lake bottom to a depth of 6 or 8 inches is the most accurate method. I am not sure as yet just how practical it is, as I did not get entirely accustomed to working in the helmet we had, but I am planning to continue this work next summer.

As regards the river-pollution investigation, Mr. Wiebe, who did the work under the direction of Doctor Van Oosten, will speak for himself. It is hoped that similar work will continue. Pollution in the Mississippi is as serious as in many eastern waters.

Of the miscellaneous work of the station I want to mention that in addition to the pond investigation, which will be described by Doctor Davis and Mr. Lord, the station carried on considerable fish-cultural operations as a side line and does its part in carrying out the policy of the bureau in assisting local fish and game clubs with their problems.

Doctor Rich. I want to explain further in regard to this matter of providing fishways for dams on the west coast. The problem is rather complicated there, as Mr. Scofield can testify. What they are trying to do is to devise means for getting salmon over high dams—much higher than this one at Keokuk. The power companies, of course, are very anxious to have this thing solved, so that they can go ahead in the building of innumerable dams without interfering with the conservation of fish. The device that was tried out, and which was successful to a certain degree, was advertised widely in one of the engineering journals as solving the difficulty; but apparently the only people who had confidence in the success of this apparatus were those who devised the apparatus. However, I don't think this equipment would be applicable to the Mississippi River dams.

Mr. Higgins. About a year ago the States of Minnesota and Wisconsin became greatly agitated over the pollution of the Mississippi River, and a committee from the State legislatures was appointed jointly, an appropriation was made by the States for carrying on this work, the various State health departments were brought into investigation, the United States Public Health Service was appealed to for assistance, and assistance from the Department of Commerce also was requested. The aid of the Bureau of Fisheries was offered by Secretary Hoover, and at the last moment funds were allotted for conducting an investigation on the pollution of fishes. I might say that last June the report was to be prepared and presented to the State legislatures for appropriate action on January 1, so you can see the scope of the work was necessarily very much restricted. Mr. Wiebe has made as thorough a study of the survey as time permitted and has made a report, which will be forwarded to the State legislatures.

MISSISSIPPI RIVER POLLUTION

By A. H. Wiebe

I shall speak on the biological survey of the upper Mississippi River. The object of the survey was to determine if pollution from the Twin Cities—Minneapolis and St. Paul—is a factor in destroying life in this section of the river. The work of the survey was divided into three phases: (1) Determination of the absence or presence of fish; (2) a study of the bottom fauna; and (3) a study of the plankton organisms. The part of the river studied extends from

just above Minneapolis to just above Winona. Winona is about 110 miles below St. Paul.

The sampling stations on the Mississippi River were so distributed that data were obtained (*a*) before any sewage had entered, (*b*) after all the Minneapolis sewage had entered, (*c*) again after all the St. Paul sewage had entered, and one tributary, the Minnesota River, had entered the Mississippi River, (*d*) after the South St. Paul sewage has been added, and (*e*) at various distances below these points, where most of the sewage is added. In order to obtain comparative data from unpolluted streams, samples were taken from the Minnesota, St. Croix, Cannon, and Chippewa Rivers also.

To give you some idea of conditions in the Mississippi River below the Twin Cities, I might mention the fact that all the domestic sewage and trade waste from the cities (joint population approximately 650,000) is dumped into the river without having been treated previously. In addition to this, some 7 or 8 miles below St. Paul are the packing plants of Swift and Armour, so situated that all of their sewage goes into the river. The packing interests, of course, claim that they utilize everything except the squeal. If that is true, the squeal must have some extremely obnoxious qualities.

Within the Minneapolis area conditions are aggravated further by the Ford power dam, situated just below the city. This dam causes the settling out of most of the solid materials in the Minneapolis sewage. The city engineer claims that there already exists a 12-foot layer of sludge behind the dam, and that this deposit is increasing at the rate of 12 inches a year.

As the time is rather limited, I shall not attempt to give a detailed discussion of the survey but shall confine myself to a brief statement.

The determinations of dissolved oxygen made by Mr. Crohurst, who is in charge of the sanitary survey that is being made by the United States Public Health Service, show that for the month of August the amount of dissolved oxygen just below Minneapolis was 0.67 parts per million; just below St. Paul it was 0.87 parts per million; about 3 miles below the packing plants of Swift and Armour it was 0.51 parts per million; and at Hastings, about 36 miles below St. Paul, it was 0.39 parts per million. At Red Wing, at the head of Lake Pepin, the average for the month was 2.25 parts per million; but even here there were 11 days during the month when the dissolved oxygen was less than 2 parts per million. In the Mississippi River above Minneapolis the average for the month was 6.59 parts per million. At the lower end of Lake Pepin it was 5.37 parts per million, and at Winona 5 99 parts per million. For the tributaries—the Minnesota, St. Croix, Cannon, and Chippewa Rivers—the amounts of dissolved oxygen were 5.70, 7.10, 7.43, and 6.26 parts per million, respectively.

The bottom fauna of the Mississippi River above Minneapolis is dominated by clean-water species: May-fly nymphs, caddis-fly larvæ, planaria, and the larvæ of Simulium. In the metropolitan area and below it, as far down the river as Hastings, the bottom fauna is dominated by sludge worms (Tubificidæ). In one instance the number of sludge worms per square yard exceeded 300,000.

Red Wing marks a transition in the condition of the Mississippi River. This is shown by the decrease in the number of sludge worms and the increase in Mollusca and leeches; also by the presence of large numbers of Hyalella and a few specimens of Asellus in August and a few dragon-fly nymphs and caddis-fly larvæ in September.

At Winona, May-fly nymphs are found again along the shore. This may be taken as a sign that the river has been purified sufficiently to support the life of clean-water species.

The data on dissolved oxygen and the bottom fauna show that the upper Mississippi River is grossly polluted from within the metropolitan area and down to the head of Lake Pepin. Lake Pepin acts as a settling basin. At the lower end of the lake and at Winona the waters of the river are greatly improved. Such being the case and knowing that the tributaries are not polluted, the conclusion that the pollution of the Mississippi River is due to the sewage from the Twin Cities seems warranted.

The study of the plankton shows apparently nothing, as far as pollution is concerned. Variations in the amount and character of the plankton occur, but these variations are not sufficient to warrant the conclusions that they are due to pollution; they probably are seasonal variations.

Game fish and other small fish were abundant in the Mississippi River above Minneapolis and in the tributaries—the Minnesota and the St. Croix

Rivers. Above Minneapolis, 1,500 to 2,000 wall-eyed pike were taken in one seine haul. No seining was done in the Chippewa River or in the Mississippi River at the foot of Lake Pepin and at Winona, but the game wardens at these places reported that game fish were abundant.

No seine hauls were made in the Mississippi River just below Minneapolis, but the data on dissolved oxygen and bottom fauna make it seem very doubtful that any fish could exist there. Thirteen seine hauls made in the Mississippi River between St. Paul and Prescott yielded nothing except one stickleback. This was taken a short distance below the mouth of the Minnesota River.

The absence of fish is correlated with the presence of tolerant or pollutional bottom-dwelling animals and low concentrations of dissolved oxygen. Thompson, who worked on the oxygen requirements of fishes in the Illinois River, claims that even two parts per million of dissolved oxygen are not sufficient to support fish life. The results of oxygen determinations that I have mention are far below two parts per million.

I give below a table to show the relationship between the amount of dissolved oxygen, average number of fish per seine haul, and the number of species at stations 1 to 8.

Station	1	2	3	4	5	6	7	8
Parts per million of dissolved oxygen	6.59	6.08	0.67	5.70	0.87	0.51	0.39	7.10
Average number of fish per seine haul	1,000	750	------	330	⅓	0	0	330
Number of species	14	19	------	24	1	0	0	17

NOTE.—Stations 5, 6, and 7 include all hauls (13 in number) made in the Mississippi River between St. Paul and Prescott. Station 1 is above Minneapolis, 2 is just within the metropolitan area, 3 just below Minneapolis, 4 is on the Minnesota River, and 8 on the St. Croix River.

The results of the survey, which I have presented here very briefly, seem to point to the conclusion that the absence of fish in the upper Mississippi River from below the Twin Cities to Prescott is due to the pollution caused by dumping sewage from the Twin Cities into the river.

AQUICULTURE

GENERAL REVIEW—PATHOLOGY AND EXPERIMENTAL FISH CULTURE

By DR. H. S. DAVIS

The problems that have been under discussion at this conference, with few exceptions, have been confined to the so-called commercial fishes. To my mind, the game fishes are in many ways fully as important as those that form the basis of the extensive marine and fresh-water fisheries. True, from the monetary standpoint they are not to be compared with the more abundant commercial species, but the importance of the game fishes in connection with the life and health of the Nation far transcends their market value. In evaluating the relative importance of these fishes, statistics are of little avail. In the first place, we have very few statistics on the catch of the game fishes; and even if we had, they would be of little value as a criterion of their relative importance. Can you place a monetary value on the dozen trout or the couple of fine bass that are the concrete rewards of a day spent on a woodland stream or the more open waters of a beautiful lake?

Not only do the standards by which we must gauge the relative importance of these fishes differ, but the scientific problems they present are in many cases quite different from those that characterize the commercial fishes. Inasmuch as the game fishes are predominantly fishes of our inland streams and smaller lakes, they are more susceptible to the influence of man, either for good or ill. Anyone who goes angling for trout or bass in these degenerate days does not require a knowledge of statistical methods to determine that the fisheries are depleted and that overfishing is a fact and not a theory. There is no doubt that the situation in many localities is a most critical one; and while the causes are fairly well understood, it may not be easy, in any particular instance, to lay one's finger on the most important factor involved in the depletion. But it is not my purpose to consider the causes of this depletion,

but rather the corrective measures that must be enforced if our game fishes are not to suffer the fate of the passenger pigeon and the dodo.

For various reasons, the bureau's efforts in this direction are confined at present to the artificial propagation and rearing of the more desirable game fishes. While it is obvious that the scope of this work should be enlarged and broadened, owing to the limited time at my disposal I shall confine my remarks to the problems that are at present under investigation or are to be taken up in the near future.

Necessarily, the program we have adopted has been dictated largely by expediency, and no one realizes more clearly than I that from the strictly scientific standpoint it has many serious defects. In outlining the work that we are attempting to carry on the primary purpose has been to render the greatest possible assistance to the fish-culturists in their efforts to increase the efficiency of the hatcheries. That there is urgent need of improvement in fish-cultural practices is obvious to anyone who is familiar with the work of the hatcheries. This statement is not intended as a criticism of present-day fish-cultural methods, but rather to emphasize the fact that, owing primarily to our ignorance regarding many of the factors involved, our hatcheries are not getting the results that can reasonably be expected of them. This fact has recently led Doctor Knight, of the Biological Board of Canada, to advocate the elimination of the hatcheries, although I have not as yet been able to obtain a clear idea of just what he proposes to substitute in their place.

The work in trout culture, which we are now carrying on, is the direct outgrowth of the work carried on several years ago on the diseases of the young fish. It did not take us long to decide that if we were to get anywhere regarding their control it would be necessary to consider many factors that relate to the diseases. Control measures must be from a prophylactic standpoint rather than therapeutic. As the food appeared to be one of the most immediate factors indirectly concerned in the outbreak of certain diseases, it was decided to study that phase of the problem. Feeding experiments were started in the summer of 1923 at the Manchester (Iowa) hatchery. The following year the experiments were continued at the White Sulphur Springs (W. Va.) hatchery. While we learned a great deal from our experiments, we found that if we were to get the best results it would be necessary to establish a station where we would have complete control of the fish and where the production of fish for planting was of secondary importance. We were able finally to establish a hatchery for this purpose at Holden, Vt. I suspect that Mr. Leach was willing, because it was a hatchery with a past, and a very shady past at that. There are advantages and disadvantages to this hatchery. In the past there has been a heavy mortality early in the spring and also during the summer. However, it also has some advantages. Anyone can raise fish where there are no troubles at all. We feel that it is a distinct advantage to have a hatchery all our own, so that we can continue our experiments indefinitely and where we will not be obliged to confine our work to young fish, as would be necessary if the work were taken up at a regular fishery station.

Mr. James will tell you of the details of the feeding experiments later on. I might say that we have been trying the various kinds of food in use at hatcheries. We have been determining the efficiency, relative value, etc., of these foods for trout of different species, and we have positive evidence that the same kind of food is not always equally good for all species of trout. Each species must be considered by itself. At the Holden station we have been able to carry on feeding experiments with all except the brown trout. I have recently received an inquiry from a leading fish-culturist regarding food for brown trout, but could not be of much assistance to him, as we have not been able to experiment with this species. Of course, this food problem can be carried on almost indefinitely, because there are large numbers of foods that are in common use, and each hatchery has its pet foods which it thinks are far superior to every other. For a single species of trout there are certain foods that are much better than others. One thing definitely certain is that no artificial foods (such as liver, heart, and foods of that type) are equal to the natural food. One line of experimentation that we are working with now is concerned with the possibility of producing some natural food that can be used to supplement the artificial foods on which these fish must depend for economic reasons.

Turning from the feeding experiments, I might touch briefly upon the diseases in hatcheries. This is a problem that will always be very important. When you consider that it is absolutely necessary to crowd these fish together in

comparatively small areas, you will see that the opportunity for the spreading of various diseases, which under natural conditions usually cause little injury, becomes an important factor. The most serious disease we have to deal with in the case of the young fingerling is caused by a protozoan—a flagellate—which in certain stages of the life cycle attacks the epithelial lining of the intestine and causes extensive destruction of the epithelial walls. Death of the host quickly follows. In another stage it lives as a free-swimming flagellate in the lumen of the intestine. In this stage it causes very serious effects. The host first becomes emaciated, growth is stunted, and it gradually becomes weaker and weaker, and eventually may die. I have often wondered how they could stand such a severe infestation without showing more serious effects than they do. Sometimes the entire contents of the intestine appear as a writhing mass of these organisms.

Last summer at the Holden station we discovered two new diseases—one a gill disease, due to bacteria, which appear on the surface of the gill. The long filamentous bacteria are found lying side by side in a continuous layer, so as to form a film over the gills. As a result of the irritation produced by the bacteria, the epithelial membrance becomes thickened and the adjoining filaments may fuse into a common mass. Of course, as a result of this thickening of the epithelium and the bacterial film covering the gills, the intake of oxygen through the gills is interferred with seriously, even if there are no other detrimental effects. The disease may cause heavy mortality. It has been found that it can be controlled easily by using a solution of copper sulphate, which is very effective in many diseases of this character.

The other disease to which I have referred affects only the fins. This, again, is probably a bacterial disease, though our investigations in this case are not so far advanced. It probably can be controlled by copper sulphate solution, as in the case of the disease that attacks the gills. One of the great difficulties in studying diseases at hatcheries is that usually we do not find out about them until they are far advanced. By the time the mortalities become heavy enough to attract the attention of the fish-culturists the diseases are so far advanced that most of the fish die. We should be able to recognize these diseases in the early stages. I think we can make better progress in controlling these diseases by thorough study of them at Holden than by relying on reports received from superintendents of the hatcheries. I might also say in this connection that it is our purpose at this hatchery not to run it in a purely experimental manner. What we hope to do is to conduct it in much the same way as is followed at the other hatcheries—crowd the fish, etc. We are developing a practical hatchery, with thousands of fish, such as we have in various Federal and State hatcheries. In this way (reproducing hatchery conditions) we shall get a picture of what goes on at the hatcheries throughout the country.

Another line of investigation is the method of handling the fish. We have not attempted much in this field, other than to carry on some experiments as to the effects of light on fish. Results indicate that it is better to protect young trout from exposure to direct sunlight. Mr. James will tell you more about this experiment.

Another problem on which we are just beginning is that of selective breeding. The more I think of it, the more I wonder why it was not taken up long ago. There are great possibilities in this direction. The Japanese and Chinese have been doing it for some time with goldfish. There is no doubt but that trout that have been reared in the hatchery for several generations are often more adaptable to hatchery conditions than are the wild fish, and that undoubtedly is due to more or less selective breeding, which is inseparable from ordinary hatchery practices. We are confining our breeding experiments to brook trout. What we are trying to do is to get a strain of trout that will grow rapidly and mature early, producing the maximum number of eggs. Later on, we hope to extend the work to other species.

Another problem that we are attempting to investigate, but on which we have not made much progress, is to determine the survival of the fish from the hatchery when planted in streams. This can be correlated closely with another problem—the best age at which to plant trout. This is a perennial problem that has been cropping up for years. No two fish-culturists have the same idea on the subject. The questions revolve around this one point—what is the proper size or age at which to plant trout? As I see it, it is purely a business problem and perhaps can be expressed in another way; that is, how can you produce 1,000 fish in a stream, above the legal size, with the least

expense? Perhaps by planting 10,000 fry, or 5,000 fingerlings, or so on—who knows? It is a problem that has direct economic bearing on the conduct of the hatchery. It is an expensive proposition to rear trout to the larger sizes. The mortality is heavy. Food costs are large. The expense of planting these fish is heavy. The tendency in the East is to plant larger and larger fish, because they believe (and I think there is some ground for that belief) that the larger the fish, the better the results that you get. Whether this is sound from a purely economic standpoint, I do not know. In some places they are planting trout 8 and 12 inches long, and the angler comes along the next day and catches them.

I shall now pass to pond culture, which is quite a different problem, in many phases, from that of trout, because the methods used are necessarily very different. Pondfishes are such fishes as the bluegill sunfish, the bass, and the crappie. We must produce spawning grounds and allow them to live under normal conditions. Natural conditions are desirable if we are to get as many fry and fingerlings as possible. Experimental pond work has been taken up only very recently. Last year the Fairport station had 21 ponds, the facilities of which were devoted to experimental purposes. Mr. Lord, who has been conducting these experiments, will give more details. I want to state, very briefly, the general program on which we are working. One of the first problems, and one that I consider of major importance, is the production of food for these fish. So far, we have not attempted to use artificial food. I think we will modify this when we have gone a little further into our operations. What we are trying to do is to produce the maximum amount of fish flesh per acre. We must use much the same methods that the agriculturist uses in producing crops on land. As you are probably aware, it has been found that we can produce as much flesh food, per acre of water, as we can on land, and possibly can do even better than that. Our pond experiments in the production of food revolve around fertility. Of course, this brings up the problem of the best fertilizer, the time when it should be applied, and so on. There are a multitude of problems, all of which we know comparatively little about. In connection with this problem is the study of forage fishes. By forage fishes we mean small fishes, such as minnows, on which the game fish feed. This is an immensely important problem and one on which very little has been done. During the past year we have been working with a number of species of fish at the Fairport laboratory and we have obtained some very encouraging results. I think there are great possibilities in the development of the use of forage fishes in pond culture.

Then, of course, just as in the case of the trout, we are undertaking investigations of parasites and diseases of these fish. These problems are not so serious as in the case of the trout, because the fish are not so crowded, but they will probably become more so in the near future. Already we have gotten quite a bit of publicity with regard to the parasites that occur in bass. One important parasite is a tapeworm, known as *Proteocephalus ambloplites*, which occurs in the bass. The adult stage does not do much harm, but the invasion of the parasite in the larval stage produces serious results, for it migrates into the connective tissues. It seems to have a special liking for the gonads, both in the male and the female. The result is that the fish are virtually sterile. They may appear to be all right in every respect, but they do not produce young. This parasite was first found in hatchery fish at the Neosho (Mo.) station, and since then has been found to be quite widespread. It probably was introduced into our hatcheries in fish shipped from Lake Erie. This illustrates one of the handicaps under which the fish-culturist is laboring because it is necessary, in order to get a supply of fish for the hatcheries, to ship them very long distances. Lake Erie was a common source of supply for the smallmouth bass, particularly, and as a result this parasite has been introduced widely throughout the country. This parasite probably is the most serious enemy with which we have to deal, in connection with bass, at the present time. Here, again, we are planning to start some selective breeding experiments. This work will involve difficulties that are not present in the case of the trout because the pondfishes are not so closely under our control. I believe, however, that it is going to be possible to produce superior strains of fish, which will be better adapted to hatchery conditions. Furthermore, more attention should be paid to the strains of wild fish that we are introducing into our hatcheries. If possible, we should use only superior strains of wild fish in building up a brood stock.

Another problem that we are intending to undertake this year is to look into the advisability of using fish in pond-cultural work which have not yet been utilized for this purpose. There is an urgent demand for a fish that

grows rapidly and which will adapt itself to conditions existing in small ponds. The bluegill sunfish is the best that we have for this purpose at the present time. The yellow perch is very good and has some possibilities. However, certain objections have been raised to the use of the yellow perch for this purpose.

Still another problem which, I think, offers possibilities is that of utilizing two or more types of food by having more than one species of fish in the same pond at the same time. The hatcheries throughout the country usually have only one species of fish to the pond. It is possible that two or more species could be utilized in the same pond, and we could get a greater poundage from our ponds in that way. This involves the utilization of all food in the pond. We are approaching this from two angles—by the introduction of forage fishes and by the use of two or more ecologically dissimilar species of food fishes. We can introduce one type that feeds on the bottom material and another that feeds on the plankton. In the past it has been a matter of utilizing one food while others were allowed virtually to go to waste. It seems to me that if we once know the practicable combination, we can utilize all these various sources of food and thus get a much larger yield.

Doctor Van Oosten. The black bass taken in commercial seines are retained in live boxes until collected by the State. Shortly thereafter a white film covers the fish. I have seen as many as 25 to 50 per cent of those fish with white coatings over the eyes. What do you think is the cause

Doctor Davis. Does it spread to various parts of the head?

Doctor Van Oosten. No.

Doctor Davis. It might be a bacterial infection.

Doctor Van Oosten. These are in Lake Erie.

Doctor Davis. That is the only thing I can think of at the present time. It would be rather remarkable that this is confined only to the eyes. It would seem that the fins would become infected if it is the species I have in mind. There was found, of course, recently a number of worms in the eyes. In fact, they have taken several hundred out of the eyes of a single fish, but I think this would hardly be found in this case.

TROUT CULTURE AT THE HOLDEN EXPERIMENTAL STATION

By M. C. James

Virtually all of our work bearing on the scientific aspects of fish culture must be done at the hatcheries themselves. Formerly we were forced to move to one or the other of the bureau's stations, shipping equipment back and forth and making the best of the facilities afforded for undertaking the investigations. While every possible cooperation was offered by those in charge of the stations, their first object was the production of fish, and our efforts sometimes involved the destruction of fish and required space and necessitated changes that might reduce production materially. The net result was a failure to achieve the fullest results from the scientific work. Consequently the opportunity to secure from the division of fish culture a going trout hatchery, to be under our control with no obligations for the production of fish for distribution, was welcomed. The Holden (Vt.) substation was turned over to us in 1925. It probably is the only trout hatchery in the country to be devoted solely to scientific investigations of fish-cultural problems.

Parenthetically I may say that within the last few days there has appeared in the latest issue of the Scientific Monthly an article, by Dr. Nathan Fasten, which sets forth, far more concisely than I could hope to, the exact nature of these fish-cultural problems. He cites 12 separate phases, and I am tempted to refer to a few of the more important ones as an exact definition of the purposes of our work in this field.

Returning to the Holden station, it is sufficient to say that there is a 40-trough hatchery building with a room serving as a combination office and laboratory, and necessary outbuildings. There is an excellent water supply—both spring and brook. Of outside pond and raceway facilities there are none.

and the supplying of these deficiencies by the construction of 9 pools and 285 feet of raceways during the last two years has accounted for much of the time spent there and for the somewhat limited output of a strictly scientific nature.

A few words as to the conduct of such an enterprise may be permissible. It is probable that the keeping of exact, easily analyzed data and records on every condition occurring in the ordinary State or Government hatchery would bring to light information that would surprise the keepers. Such information as is generally recorded is fragmentary, unsystematic, and frequently an approximate expression of personal judgment. The fish records cover the lots as groups, and analysis of diverse conditions is impossible. At the Holden station it is a cardinal principle that every factor having any bearing on the fish must be recorded. A card filing system has been begun to give ready information on the history and status of virtually every fish on hand. These are cross indexed so that the fish in any trough or pond can be traced back to their origin with all deviations from normalcy exposed; or any particular lot of fry or eggs can be followed through to the final disposal of the last survivor.

Routine records are kept of losses, temperature, where changes are occurring, growth (if desired), diet, etc. Any unusual manifestation in any lot is noted as to time and place; this has proved to be of value in checking up in the case of a subsequent outbreak of disease. In fact, such a plan will give, in time, a mass of information, from the ordinary routine of the hatchery, that will be as valuable as any to be gained from formal experiments. It may be stated that with 6 species on hand from a dozen or more different sources, distributed among 10 ponds and 15 raceway compartments, as well as 40 troughs, a season's haul of notes can well be described as voluminous.

The feeding experiments cited at the beginning are part of a series initiated four years ago and carried on at other hatcheries. Based, at the start, on a belief that some of the many difficulties besetting the rearing of trout might reasonably be expected to have their origin, to some degree, in the diets of artificial culture, the assumption of vitamin deficiency was entertained. First attempts to improve such a condition (if it existed) by the enrichment of various foods with substances of high vitamin value, like cod-liver oil and yeast, were not highly successful. The most encouraging results were obtained in the augmentation of the diet of rainbow trout with vitamin A from cod-liver oil and vitamin B from yeast and natural sources. A short time later, investigations by other workers brought out the fact that liver, heart, etc., were much richer in vitamins than had been suspected. Less emphasis has been placed on this branch of the work of late; it has been developed that under some circumstances a vitamin enrichment of the diet of rainbow trout, by means of cod-liver oil, has been of some benefit, but our experiments have consistently shown the reverse to be true in the case of brook trout.

The more recent work (that of the past season) was devoted to determining the effects of various diets on a serious disease that prevails at Holden; to determining the relative values of the common diets of present practice (beef heart and liver and sheep liver); and to the development of three new products to supplement the meat diets. Beef liver was found to be apparently better, as regards lower mortality and increased growth, when compared with the other meats mentioned. The superiority is more marked, of course, where fish are to be reared to large size rather than planted as early fingerlings. The new products utilized were soy-bean oil meal, a dried shrimp, and a clam meal. It is widespread practice among fish-culturists to feed their larger fish a mixture of fresh meat with a large proportion of a cheaper meal cereal, such as middlings or a low-grade flour. There is a serious doubt that the carbohydrate of these materials enters into the nutrition of trout very extensively, and the purpose of the products mentioned above was to obtain the roughage values obtained from cereals, and at the same time feed a high protein ration to further growth and condition, and at the same time supplant part of the expensive meat. It can be said that experiments on a small scale have shown no objectionable features in these substances, with some promise of real benefit in the case of the clam meal, and their use will be continued in a more practical way.

The procedure and technique of these feeding experiments do not appear to have the complexity that would warrant the application of the term "scientific." The simple process of feeding one lot of fish a certain diet and another lot a different diet, and observing their reactions, growth, and mortality, would not seem to require a highly specialized training or a broad erudition. This is true; but at the same time it must be recalled that our present knowl-

edge of vitamins has been gained from a multitude of experiments no more complicated than this. The work with fish enjoys certain advantages and suffers certain handicaps, in comparison with the vitamin investigations in higher animals. From a statistical standpoint, the results gained from experiment with a lot of 1,000 fish are worth more than those gained from a single individual or 5 or 10 rats or pigeons. At the same time, the vitamin investigators have a "standard rat" to work with, as well as uniform specimens of pigeons, etc., and they exercise almost complete control over the environmental conditions in which their animals exist. There is no such thing as a "standard trout," and the great fluctuation in the growth rate of fingerlings, the frequency and severity of epidemics, the changes in water conditions, the cannibalism, the inability to keep an accurate check on food consumption, combine to furnish the fish dietitian with food for thought for his idle moments.

Concerning the subject of fish breeding, I can offer little more than a prospectus at present. Briefly, this is merely the application of the universally known principles of selection of parent stock for desirable qualities, such as man practices wherever he is rearing animal or vegetable life for his use. Strangly enough, application of these simple principles has been merely rudimentary. At all hatcheries there has been, consciously or unconsciously, a selection of the future brood stock from the better grade of breeders, but the practice has been spasmodic, applied to large groups chiefly, little accurate check of results has been kept, and selection for specific qualities has been largely ignored. The commercial dealers probably have accomplished the most in this field, and while their efforts and achievements have been commendable, the need for immediate monetary profits necessarily has circumscribed their efforts.

We have been forestalled, in our plans to inaugurate a program of scientific selective breeding, by an undertaking at the New Jersey State hatchery. Doctor Embody, of Cornell University, is directing this project. Briefly, they now have a fourth generation of selected brook trout. The original stock consisted of survivors from a large number of hatchery fish that had been subjected to all the vicissitudes of ordinary hatchery life, including a serious epidemic. Enough remained to permit the work to proceed on a similar basis for the succeeding generations. Summarized, they have cut down the mortality, at the July fingerling stage, from 98 per cent to 30.8 per cent in the 1925 generation. At the same time, the average length at this age has increased from 2 to 3½ to 4 inches. Thus, the primary selection was exclusively for disease-resisting characters, and this has been adhered to throughout, although rapid growth has entered into the selection from the later generations as a secondary character. This has been accomplished by what is really mass selection with no attention paid to individual qualities. Incidentally, I know of no publication, save the report of this work, that treats of the selective breeding of trout, except in a cursory way, and that sets forth a definite program and describes the results of that program in a definite, reliable, and comparable manner.

As stated, efforts in this field at the Holden station are chiefly aspiration. This fall we took eggs from a lot of brook trout that have shown rapid growth and early sexual maturity. This is the first yield of eggs from these fish, and we will be in position soon to determine whether this precocity has a definite influence on the vitality of the eggs or fry. This has been mass selection, as the early-maturing character was common to all the fish that spawned, and no other outstanding differences existed. We also have one lot of this year's fingerlings, which exhibits the disease-resisting character to a greater degree than does the general hatchery stock. These probably will not yield eggs in any quantity until 1928. There is also on hand a lot of this year's fingerlings, which has exhibited splendid growth, and these will be depended upon as parents of a strain that emphasizes this quality. As for individual selection, we have this year taken eggs from five or six females that showed desirable qualities in the way of color, form, and production of a large number of eggs. These eggs will be segregated, of course, and the lots will continue to be handled thus until maturity. The fish spawned from individually selected stock are marked by numbered tags for future recognition, and the best males are selected for pairing with these fish whenever enough of the latter are ripe. However, extensive attempts to establish pedigreed stock will be deferred until the general strains have become well established.

This limited experience has demonstrated already that the demands of this sort of work, as regards time, effort, and space, are heavy and that the wisdom of having a hatchery devoted exclusively to this type of investigation is unquestionable. It is evident that the selective breeding experiments will require several years in the immediate future that will be barren of results; but the ultimate outcome will be of fundamental value. If it is not already evident. I should state in closing that all of the work at the Holden station is extremely practical and has as its aim the immediate application of all discoveries to the improvement of fish cultural practice.

POND CULTURE

By Russell F. Lord

Doctor Davis already has given an idea of the program we have initiated in the past summer at the Fairport biological station. Although fishponds may be small, pond culture is a large subject, notwithstanding. Many elementary and practical questions concerning the propagation of our pondfishes have not yet been answered satisfactorily. Here are some examples:

What is the best number of fish to rear in a pond of certain size?

Should adults and young be raised together, or should young fish be removed to rearing ponds?

If the latter, at what stage in their development and by what method?

Should species be isolated in separate ponds?

If this is not necessary, what species can be raised together best?

Is the use of fertilizer practical?

If so, what kind of fertilizer should be used, in what amounts, when should it be applied, and for what reasons?

Questions thus pile up; questions as to the best varieties of aquatic vegetation, questions concerning the relation of aquatic vegetation to plankton and other natural food, questions as to the use of forage minnows—in short, a great problem of ecological relationship.

Various experimental data, from 22 ponds, are being collected at Fairport in an attempt to answer these questions. Not a single conclusion has been reached so far as to any of these. They could not possibly be settled in a short time. Indications, however, are numerous.

One phase of these experiments consists of systematic observations on each pond, in which records of the amounts of net plankton and nanoplankton per unit volume of water, water temperature records (surface and bottom twice daily), pH determinations, turbidity readings, and chemical determinations of dissolved oxygen are being secured and filed. A check has been made on the rates of growth of different species in the different ponds, also. Commercial fertilizer has been applied for varying periods and in varying amounts for certain series of ponds.

This is the briefest possible review of this phase of the work and does not attempt to go into any of the details. On the forage minnows, however, I shall spend a little more time. Perhaps if I tell you some of the things contained in a more or less complete report on one of the species experimented with, it will give you an idea of how the work has been carried on.

It was planned to experiment with various minnows in the hope of finding several species that would be suitable as forage fish in the bass ponds. The golden shiner (*Abramis crysoleucus*) and the black-head minnow (*Pimephales promelas*) were the only two suitable species that could be secured in numbers sufficient for practical experiments. Work was done also with the common goldfish.

Pond No. B 13, of 1,482 square feet (0.034 acre), was used as the rearing pond for the black-head minnows. This small pond had been wintered dry. On April 23, 4½ pounds of fertilizer were placed in it and the water turned in. On June 5, 4½ pounds of fertilizer also were placed in it, and then from June 19 to August 30, inclusive, 2 pounds of fertilizer were applied at approximately five-day intervals.

On May 1 the pond was stocked with 72 adult black-head minnows. On May 15, 19 more were introduced, bringing the total up to 91 adults for the pond. These fish were secured in the sloughs of the Mississippi and Cedar Rivers. The pond was watched carefully for the first signs of spawning.

On May 21 a piece of floating wood was found in the pond with many hundred closely arranged eggs adhering to the under surface. The movements of well-developed embryos could be seen through the shells. A sample of these eggs was taken to the laboratory and more closely examined. Hatching began in the afternoon and was still in progress on May 22. Almost all of the emerging fry, however, were weak and dying, and before night all that had hatched, as well as the unhatched eggs, were all dead in the jar of water in which they had been placed. Fortunately, the nesting habits of this fish made detailed observations possible.

After the discovery of the first eggs on the floating wood, several boards, about 3 by 10 inches in size, were placed along the banks of the pond, about 8 inches under the surface. It was thus easy to take up each board for examination and then replace it in the soft pond bank. These nest boards were given numbers, and daily observations were made. During the season it was possible to examine 20 nests; and several other nests, out of reach on the under surface of the various pipe and platform supports, were indicated by the actions of guarding fish. The last eggs were observed on August 6. The following data was secured from observations on the 20 nests mentioned above:

Time of first appearance of eggs to first eggs eyed, average four days.
Time of first appearance of eggs to first eggs hatched, average six days.
Time of first appearance of eggs to total eggs hatched, average nine days.

The eggs of the blackhead minnow thus required a period of six days (at the temperatures given for the pond) from the time of their first appearance to the time of hatching. The eggs were not, however, all placed upon a nest board at once, but sometimes in as many as four successive lots. It was found that the eggs eyed and hatched in the exact order of their appearance upon the nest boards. No attempt was made to see if all the eggs on a certain nest were from a single female. Two fish, however, were all that ever were seen near a nest at the same time.

The habits of adult blackhead minnows were especially interesting. One or both of the parent fish were always seen near the nest board. Usually they kept immediately under the eggs and were very active, moving fins, tail, and body constantly. The male fish was the more aggressive, and when attempts were made to catch various males with a dip net, they would swim under, over, and around it, but refused to be frightened away from the nest permanently. When two fish were present, the female kept quietly under the nest board, at the shore end, and always returned to her position.

Thus, the protective instinct seems to be highly developed in these minnows For example, on May 28 two males were found fighting vigorously. The scene of action was close to the nest of the larger fish, which had been observed often enough to be recognized easily, and hostilities evidently began when the smaller male discovered the loss of his entire nest and went in search of it. This nest had been removed to another pond to see if the eggs would hatch without parental protection. The male on guard at nest D, however, resented the inquiring stranger and was quick to defend his own eggs. The two fighting males had a firm grip on each other's jaws and were shaking their heads and bodies strongly. As they approached the surface of the pond both fish were scooped up with one dip of the net and examined. Both were male *Pimephales promelas*. The color was deep black, especially about the head, with two large bands of gold from the belly part way up the sides, and a third, smaller patch of gold near the belly side of the peduncle. Tubercles were prominent on the snout. Both of these fish were returned, uninjured, to the pond.

On other occasions the guarding males would nibble at investigating fingers whenever a nest board was touched. They appeared to become more fearless as the hatching time approached. This display of aggressiveness seemed to be essential, as many times water beetles and water bugs, both adults and larvæ, were taken in the act of destroying the eggs on some nest board. In fact, when nests were transferred to other ponds where these beetles were abundant, it was necessary to protect them with fine wire screen.

The growth of fingerlings also was noted with interest. A large sample of blackhead minnow fingerling was measured on August 17. As the spawning had been more or less continuous from May 21 to August 6, it was not surprising to find the fish varying in size from 8 to 40 millimeters. Most of the

larger fingerlings easily avoided the small hand seine that was used in the collecting, but the fish caught indicated a fairly even distribution, from the very smallest to those in the 40-millimeter class. The actual body lengths and the distribution for these lengths follow:

Body length, millimeters	Total number of fish at this length	Body length millimeters	Total number of fish at this length
8	1	17	7
9	5	18	4
10	11	19	1
11	4	20	4
12	5	21	1
13	4	24	1
14	4	25	4
15	14	35	1
16	7	40	1

As to plankton, samples of net plankton were collected from May 8 to September 15, inclusive. The May samples showed the highest concentration of the season, and even this was very low in comparison to most of the other ponds. The supply of net plankton continued to be very scarce throughout the summer. This was not surprising, considering the great number of young fish so small a pond was supporting.

Nanoplankton was collected from July 31 to September 15, inclusive. The amount of nanoplankton was several times that of the net.

There was a good supply of aquatic plants in this pond. Potamogeton was abundant at one end of the pond and submerged Ceratophyllum and Elodea also were common. A jellylike blue-green algæ that floated on the surface on hot days also was very abundant. A scum of the "jelly" had to be removed from the surface of the pond on two occasions. Other algæ (filamentous forms) also was abundant, but no attempt was made to study the algæ of this pond in any detail. Aphanizomenon, so abundant in other ponds, was not present in B 13.

The miscellaneous records taken at regular intervals include water temperature, pH, turbidity, and oxygen content.

Pond No. B 13 was drained on October 14 and the fish were removed. The minnows were first culled by allowing the smaller fish to pass through the meshes of a net that retained the larger. Four hundred and fifty-eight large minnows were thus separated and counted. Only one fish of this entire lot was clearly an adult. The rest of the minnows were very uniform in size and were no larger than blackhead fingerlings hatched in the first week of June. The smaller fish ranged from 12 to 41 millimeters in body length at a weight of 500 fish to 3 ounces. The larger minnows were not weighed, but in comparison to D 8 bass fingerlings, these minnows would easily average 7 ounces to 100 fish. There were 6,500 of the smaller minnows. Production for the pond was 6,500 small fingerlings at 39 ounces, 367 large fingerlings (458 less the original 91) at 25.69 ounces (estimated), 6,867 total fingerling production, weighing 64.69 ounces.

Production per acre was computed at 201,971 fingerlings of all sizes weighing approximately 119 pounds (smaller minnows at 71 pounds 11 ounces and larger minnows at 47 pounds 4 ounces).

What are the possibilities of this minnow as a forage fish for bass? Of course, a single summer's observations of the life history and general habits are not sufficient for exact conclusions, but the present observations do indicate that it is a valuable fish. The following facts are cited:

1. After a size of 25 to 30 millimeters was reached, the fish were observed feeding on algæ to a great extent.
2. The species was prolific, having increased in numbers about seventy-five times by the end of the summer.
3. It spawned on boards placed for the purpose, and thus its distribution to other ponds was made easy.
4. The spawning season was of long duration. This would supply both small and large bass with a supply of fish food throughout the growing season.
5. It appeared to be relished by the game fish.

A few trials made during the summer showed that bass and crappie readily took to these minnows as food. A large nest of eggs in hatching condition were placed in a crappie pond on June 24. When the pond was drained on September 21 there was a survival of only 4 minnows out of hundreds that

must have hatched. The crappie from this pond were reported to be in better condition than in other years, and we might conclude that the minnows were somewhat responsible for this.

Another nest placed in bass-rearing pond No. E 1 had no survivors; and from 1,000 fingerling black-heads (estimated) placed in another pond, none were recovered in the fall.

The present conclusion is that this species will be of value in pond culture. Its superiority or inferiority to other forage minnows must be determined, however, by more experiments.

Good results also were obtained with the goldfish and golden shiners. In fact, the success met with the latter species was the factor that brought our production of bass fingerlings per acre up to an average of 5,000 fish.

In the rearing ponds where golden shiners were used in numbers the production of bass fingerlings was over 6,000 per acre.

As each of the 22 ponds must be taken up in this same way, we have so far collected a large number of miscellaneous data. It is evident that our work must be continued systematically and new experiments instigated as rapidly as possible. It is now necessary to make a thorough study of our material in an effort to establish correct interpretation and correlation.

Mr. NESBIT. Mr. Lord mentioned that he had in mind a number of other investigations. We must realize that he had just one summer for all this work. There are one or two points that seem to me rather significant—regarding the efficiency of different depths in the various ponds used. In this connection I may say that the idea in fertilizing ponds, in addition to what food might already be supplied, is that you modify the growth of the organisms; but it seems to me that you should get the actual value of the sunlight effect on the organism you are trying to rear.

In the days when I was a botanist, I recall that the University of Nebraska was carrying on studies in the sand lakes of Nebraska on the sunlight at different levels in the water. The outstanding fact developed was that the ray of sunlight that affected the organism most did not penetrate far below the surface. It seems to me that in deeper ponds you would have to use more fertilizer, and fertilizer costs money. On the other hand, if you reduce the depth of the pond you would have conditions that may be unfavorable for the fish, so, of course, some sort of balance would have to be reached. I might add that in my own experience in rearing algæ the amount of sunlight seemed to have a remarkable effect on the species.

Mr. LORD. We had not considered that in this summer's work. The ponds are not very deep and when we applied fertilizer we just scattered it along the edges of the pond. Of course, a complete analysis of the result of using the fertilizer has not been undertaken. We have been endeavoring to cover a great deal of ground to get practical results as soon as possible.

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